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ABSTRACT

To what extent, if any, will workers in families receiving Government cash benefits reduce their hours of work? The papers by Irwin Garfinkel and by Glen Cain and Harold Watts review large numbers of studies on this question. These authors caution us against having great confidence in the detailed estimates of how workers will respond because limitations exist in all the studies. On the other hand, the reviews do give reliable guidance about the direction and order of magnitude of likely effects. According to most of the studies, one would expect prime age married men not to alter significantly their pattern of work in response to the availability of an expanded income supplement program. There is also general agreement that increasing income guarantees or benefit-loss rates would cause a moderate reduction in hours of work (in market jobs, not necessarily in the home) among married women, female family heads, and older men. In his paper, Samuel Rea, Jr. compares more than 20 negative income tax, wage subsidy, and earnings subsidy proposals. Rea uses one set of estimated relationships that specify how a beneficiary's hours of work depend on his wage rate, unearned income (pensions, rents, dividends), and those feature of income maintenance programs that influence his net wage rate and unearned income. Given predictions of worker response and data representative of the national population in 1966, Rea is able to examine how specific program changes affect budget costs, hours of work, and the share of benefits going to the lowest income groups. (Author/JM)

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STUDIES IN PUBLIC WELFARE

PAPER NO. 13

HOW INCOME SUPPLEMENTS CAN AFFECT WORK BEHAVIOR

A VOLUME OF STUDIES
PREPARED FOR THE USE OF THE
SUBCOMMITTEE ON FISCAL POLICY
OF THE
JOINT ECONOMIC COMMITTEE
CONGRESS OF THE UNITED STATES

U S DEPARTMENT OF HEALTH,
EDUCATION & WELFARE
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LETTERS OF TRANSMITTAL

FEBRUARY 14, 1974.

To the members of the Joint Economic Committee:

Transmitted herewith is a volume entitled "How Income Supplements Can Affect Work Behavior." The authors of the three studies in this volume review their own research and that of others on the question of whether and to what degree welfare benefits reduce recipients' work efforts. These studies were prepared for the Subcommittee on Fiscal Policy in conjunction with its review of the Nation's welfare-related programs.

The views expressed in this volume do not necessarily represent the views of the members of the Joint Economic Committee, the Subcommittee on Fiscal Policy, or the subcommittee staff.

WRIGHT PATMAN,
Chairman, Joint Economic Committee.

FEBRUARY 12, 1974.

Hon. WRIGHT PATMAN,
Chairman, Joint Economic Committee,
U.S. Congress, Washington, D.C.

DEAR MR. CHAIRMAN: Transmitted herewith is a volume entitled "How Income Supplements Can Affect Work Behavior." This is paper No. 13 in the subcommittee's series *Studies in Public Welfare.*

Opinions abound on whether public welfare benefits have a negative effect on recipients' work efforts. This volume contains three rigorous papers which examine facts on the subject, and summarize a body of research ranging from the results of the New Jersey income maintenance experiment, to studies of the aid to families with dependent children, unemployment insurance, and social security programs, to highly technical econometric simulations.

The results are clear enough: there *is* cause for concern, especially in view of the growing number of programs which supplement current personal consumption and which reduce benefits as earnings increase. Nearly every committee of Congress has legislative jurisdiction over one or more such program, whether the earnings-related benefits are in the form of cash, food, housing, medical care, day care, or social services. Thus, these findings have relevance far beyond traditional public assistance programs.

Subcommittee staff member Robert I. Lerman critically reviewed these papers and prepared the volume for publication. Vee Burke and Alair A. Townsend provided editorial assistance.

The views expressed in this volume do not necessarily represent the views of the members of the Joint Economic Committee, the Subcommittee on Fiscal Policy, or the subcommittee staff.

MARTHA W. GRIFFITHS,
Chairman, Subcommittee on Fiscal Policy.

FOREWORD

Whether giving money to the poor encourages idleness is a concern dating back centuries. The question often asked is, if families can live off the dole, why should the family members work? Although the basic question has changed little in hundreds of years, recent experience with welfare reform has taught us to approach the problem in an increasingly sophisticated way.

For one thing, it is now recognized that program design itself may profoundly affect work efforts of beneficiaries. In 1962, an amendment was passed which allowed recipients of aid to families with dependent children (AFDC) to deduct work expenses from their earnings before their welfare benefits were computed. Still, Congress realized in 1967 that one could hardly expect welfare mothers to work when many lost nearly every dollar of net earnings in reduced welfare payments. By passing amendments in 1967 allowing AFDC recipients to retain at least $30 and one-third of their monthly earnings (as well as work expenses), Congress attempted to encourage further AFDC mothers to take a job.

Offering financial incentives to work was a step forward, but the new amendments have proved to be expensive. The requirement that States ignore part of a family's earnings raised substantially the number of families who were eligible for benefits. For example, in States whose maximum annual AFDC benefit had been $3,000 per year for a family of four, the 1962 and 1967 amendments expanded eligibility to include recipient families whose earnings were between $3,000 and from $4,500 up to $7,500 (for those with sizable work expenses). Welfare rolls and Federal, State, and local welfare budgets soared in part because of this increase in eligibility. Instead of producing the desired effect—reductions in welfare costs through incentives to work—the amendments caused an *increase* in total welfare payments and welfare recipients and apparently stimulated little added work effort.

One lesson learned from this experience and from discussions of President Nixon's proposed family assistance plan and negative income tax proposals is that an inevitable conflict exists in the attempt to achieve some prized objectives. Another lesson is that potential effects on work effort are not limited to cash benefits, but may result from subsidized food and housing programs as well. An income maintenance program or set of programs cannot simultaneously (*a*) pay "adequate" benefits to those without income; (*b*) allow those with income to suffer only small reductions in benefits; and (*c*) keep budgetary costs low. To more nearly fulfill any one of these goals necessarily forces a retreat from at least one of the other two. To increase the financial rewards from work by allowing recipients to retain one-half instead of one-third of their earnings requires a cut in the amount provided to those with no income, a rise in program costs, or both. That such unpalatable compromises among desirable goals are necessary is becoming better understood. However, the exact nature of the compromise remains in doubt. What is the added budget cost of improving a program's work incentive features? How much added work effort would result? How many more families would be lifted out of poverty?

Often these questions are discussed in language special to negative income tax plans. The basic parameters are (*a*) *the guarantee*, or maximum grant, which is the dollar amount paid by the Government to those with no other income; (*b*) *the tax rate*, or benefit-loss rate, which is the amount by which benefits decline with each dollar of added income of the recipient; and (*c*) *the break-even point*, or eligibility limit, which is the income level at which negative income tax benefits fall to zero. It is well known that raising the guarantee improves the plight of the lowest income families at the price of higher budgetary costs and reduced work effort. Unfortunately, knowing the direction of any change is not enough. To judge one plan over another, one needs quantitative as well as qualitative information. How much increased income will the higher guarantee provide to the poorest families? At what level of added budget costs? To what extent will recipients reduce their hours of work?

Although these questions are difficult to answer with precision, they are the right questions to ask in assessing various income maintenance programs. The primary difficulty in obtaining accurate estimates of these program effects is the uncertainty about how workers will respond. To what extent, if any, will workers in families receiving Government cash benefits reduce their hours of work?

The papers by Irwin Garfinkel and by Glen Cain and Harold Watts review large numbers of studies on this question. These authors caution us against having great confidence in the detailed estimates of how workers will respond because limitations exist in all the studies. On the other hand, the reviews do give reliable guidance about the direction and order of magnitude of likely effects. According to most of the studies, one would expect prime age married men *not* to alter significantly their pattern of work in response to the availability of an expanded income supplement program. There is also general agreement that increasing income guarantees or benefit-loss rates would cause a moderate reduction in hours of work (in market jobs, not necessarily in the home) among married women, female family heads, and older men. For example, results from an OEO-funded income maintenance experiment primarily in New Jersey show that women receiving income supplements worked 10 to 15 percent less than women not receiving benefits.

In his paper, Samuel Rea, Jr. compares more than 20 negative income tax, wage subsidy, and earnings subsidy proposals. Rea uses one set of estimated relationships that specify how a beneficiary's hours of work depend on his wage rate, unearned income (pensions, rents, dividends), and those features of income maintenance programs that influence his net wage rate and unearned income. Given predictions of worker response and data representative of the national population in 1966, Rea is able to examine how specific program changes affect budget costs, hours of work, and the share of benefits going to the lowest income groups.

Rea's analysis highlights the importance of considering how much income maintenance plans reduce hours of work. Consider four negative income tax plans, each with a $2,400 annual guarantee for a family of four, but with benefit-loss rates of 100, 67, 50, and 33 percent. Using 1966 data, Rea estimates that reducing the benefit-loss rate from 100 to 67 percent would cause budget costs to rise from $2.5 to $4.6 billion if the change did not alter recipients' work output. When effects on hours reductions are taken into account, budget costs rise to $7.4 billion for the 100-percent plan but only to $5.9 billion for the 67-percent plan. Surprisingly, lowering the benefit-loss rates can actually *reduce* costs while extending coverage. But further improvements in work incentives cause progressively steeper increases in cost. Lowering the benefit-loss rate from 67 to 50 percent produces a further budget cost increase of $2.6 billion (from $5.9 billion to $8.6 billion) while a rate reduction from 50 to 33 percent further raises budget cost estimates by $7.2 billion (from $8.6 billion to $15.8 billion).

These and other numbers drawn from Rea's paper illustrate the importance of considering the quantitative dimensions of program tradeoffs. The estimates themselves should not necessarily be considered as authoritative since they are based on 1966 data and on only one set of labor supply relationships. Rea's primary contribution is focusing attention on the right questions to ask when comparing income maintenance programs.

The three papers in this volume are placed in order of increasing technical detail. General readers will find the paper by Garfinkel to be readily understandable. A greater technical background is required for the Cain-Watts paper.

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INCOME TRANSFER PROGRAMS AND WORK EFFORT: A REVIEW

By IRWIN GARFINKEL*

INTRODUCTION

While static economic theory suggests that most income transfer programs lead to reductions in the work effort of program beneficiaries, the theory tells nothing about the magnitude of such reductions.[1] How much less beneficiaries would work as a result of the enactment of an income transfer program is an empirical question. The purpose of this paper is to review critically the empirical evidence on this question. A major theme of this paper will be that the available evidence does not permit precise estimates of *how much less* program beneficiaries will work. Rather, the evidence confirms some qualitative predictions about relative magnitudes which are derived from economic and sociological theory, and it allows us to place wide quantitative bounds on work reductions induced by transfer programs.

The question of beneficiaries' work response is critical to the shaping of income transfer programs. It is sobering to recall that the very first parliamentary act which dealt with poverty, the Statute of Laborers in 1349, actually forbade *private* alms-giving to the able-bodied poor.[2] The rationale was that such aid encouraged idleness and other supposedly related moral vices. Although our methods of dealing with poverty have changed considerably within the last 6 centuries, acquaintance with British and American poor law history, or with the current debate in this country over welfare reform, is sufficient to establish the continuing importance of the question of beneficiaries' labor supply.

There are at least two reasons for the concern with the work disincentive effects of income transfers—cost and morality. First, nonbeneficiaries have a direct monetary interest in the work response of program beneficiaries. If beneficiary family members work less as a result of the program, their earnings will fall and their grants will rise. Hence the greater the labor supply reduction, the greater the cost of the program and, therefore, the larger the taxes of nonbeneficiaries. Second, much of the opposition to transfer programs, particularly those which would aid families with able-bodied male heads, stems from fear that such programs would encourage large numbers of poor fathers to either substantially reduce their work effort or to quit work. If program costs were the only concern, we would be indifferent between (*a*) a 10-percent reduction in labor supply which resulted from all beneficiaries reducing their work by 10 percent and (*b*) a 10-percent reduction which resulted from 10 percent of all beneficiaries *quitting* their jobs. But, we are not likely to be indifferent. If a transfer program induces many poor male family heads to reduce work from 50 to 40 hours a week, or causes many wives or children in poor families to work less, we are not likely to be very upset. But because it would constitute a flagrant violation of the work ethic, we would be profoundly disturbed if such a program induced many poor male family heads to permanently quit work.

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*The research reported here was supported by funds granted to the Institute for Research on Poverty at the University of Wisconsin, pursuant to the Economic Opportunity Act of 1964. The author is grateful for the helpful comments and suggestions of Robert Haveman, Robert Lerman, Larry Orr, Alair Townsend, Harold Watts, and particularly Stanley Masters. This paper draws quite heavily upon preliminary results from a joint study by Stanley Masters and myself. I take sole responsibility however, for any errors and all conclusions.

[1] Wage subsidy programs in theory need not lead to reductions in labor supply. For a discussion of the theory see sec. I. The term "labor supply" refers to time spent in employment or in unemployment (i.e., searching for employment).

[2] For a historical analysis of the evolution of the American welfare system and its forebearer, the British Poor Law, see (11) and (24).

Because there are strong a priori reasons and supporting empirical evidence for believing that the labor supply effects of transfer programs vary among demographic groups and because we are likely to feel more strongly about the work obligations of some groups than others—for example, husbands vis-a-vis wives—the demographic groups must be discussed separately. The paper is, therefore, organized around a discussion of the empirical evidence for each of four demographic groups: Prime-aged married men, prime-aged married women, prime-aged female heads of households, and older men. Each was chosen for a particular reason. Prime-aged husbands and wives are examined because of the economic importance of their work. Although the labor supply of female heads of households is not of great economic consequence, there is a great deal of public interest in the work effort of some members of this group; namely those assisted by the aid to families with dependent children program. Finally, although society does not feel that the aged should be obliged to work, they are included in order to compare their behavior to the other groups.

Three kinds of data have been used to estimate effects of transfer programs on work effort:

(1) Most studies have used cross sectional data (data which compare different individuals at one point in time only) from sample surveys. Differences in work effort which are associated with differences in wage rates and income across individuals (or across averages in standard metropolitan statistical areas: are taken as a measure of how beneficiaries would respond to an income transfer program that changed their income and net wage rates.

(2) Data on beneficiaries of actual programs have been examined. For example, differences between States in the parameters of the aid to families with dependent children program (AFDC) have been used to estimate the effect of this program on the amount of work performed by female-headed families. Similarly, changes in the old-age insurance portion of the social security program have been used to estimate the effect of this program on the work effort of the aged. Attempts have also been made to estimate the labor supply effects of those transfer programs, such as the unemployment insurance and general assistance programs, that provide aid

to a mix of demographic groups. All of these studies are critically reviewed in this paper.

(3) Finally, four income maintenance experiments have been designed to estimate the labor supply effects of transfer programs on various demographic groups. Of these, data are available only for the first experiment, popularly known as the New Jersey income maintenance experiment. These experimental findings are discussed in the sections on prime-aged husbands and wives.

In the first section of this paper a brief theoretical discussion of the labor supply effects of transfer programs and the a priori reasons for expecting different effects among different demographic groups are presented. The second through the fifth sections present and discuss the empirical evidence for the four demographic groups. The sixth section contains a discussion of some studies based on program data that make no distinction whatsoever among demographic groups. The seventh, and final, section contains a summary and some brief conclusions.

I. INCOME TRANSFERS AND LABOR SUPPLY: ECONOMIC THEORY

The most important elements of income transfer programs that affect work incentives are guarantees and tax rates.[3] The guarantee, which usually varies with family size, is the payment to a family with no other income. The tax rate (benefit-loss rate) is the percentage amount by which payments are reduced as earnings (or other income) increase. For example, if each dollar of earnings reduces benefit payments by 60 cents, the tax rate is 60 percent. In most transfer programs in the United States guarantees and tax rates are positive, so that benefits are higher the lower the pretransfer income level and benefits fall as income rises. This is true of aid to dependent children, aid to the aged, blind and disabled, unemployment insurance, and old-age insurance (OAI) for those less than age 72. In some programs, however, tax rates are equal to zero; for those aged 72 or over, for example, benefits from OAI are not reduced no matter how much the individual earns. Finally, a transfer program can also have a zero guarantee and a *negative* tax rate. In this case, when income is zero, the payment is equal to zero. As earnings increase, instead of decreasing, the payment increases. This kind of income transfer program is called an earnings or wage subsidy [4] (see table 1). While economic theory predicts that income transfer programs with positive or even zero tax rates will lead to reductions in the labor supply of program beneficiaries, economic theory says that programs with negative tax rates can lead to either increases or decreases in labor supply.

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[3] Other program elements such as work tests may also affect labor supply but the effect of these other program features is beyond the scope of this paper.

[4] To be more precise a wage subsidy program is one in which payments decrease with wage rates and increase with hours worked. An earnings subsidy is a program in which payments increase or decrease with earnings: No distinction is made between hours worked and hourly wage rates. For our purposes the two programs may be lumped together.

TABLE 1.—*Three types of transfer programs*

Earnings	Government payment under plan with:		
	Positive guarantee/ positive tax rate (50 percent)	Positive guarantee/ zero tax rate	Zero guarantee/ negative tax rate (10 percent)
$0	$2,000	$2,000	0
$1,000	1,500	2,000	$100
$2,000	1,000	2,000	200
$3,000	500	2,000	300
$4,000	0	2,000	400

Economists assume that individuals want to do more things than they have time for; that is, that an individual's time is scarce. More important, economists assume that, other things being equal, an individual would rather use his time for a nonmarket activity such as leisure[5] than for market work. How any particular individual decides[6] to allocate his scarce time among market work and nonmarket activities depends upon his tastes, his income, and the cost of not working (that is, the monetary reward for working).

By increasing his income opportunities, the guarantee in an income transfer program enhances the beneficiary's ability to afford to work less. Given the assumptions that the individual would prefer to devote his time to activities other than market work and that there are no changes in his tastes or in the price of his not working, it follows that increases in income will lead to decreases in market work. Thus, guarantees in income transfer programs lead to reductions in labor supply. Moreover, the larger the guarantee, the greater the capacity of the individual to afford to work less, and hence, the greater the reduction in market work.

A positive tax rate in an income transfer program reduces the reward for working or, what is the same, reduces the cost in lost income of not working. To an individual with a $2-per-hour wage rate, the cost of not working an hour is $2. But a transfer program with a 50-percent benefit-loss rate would reduce that cost to $1 per hour, because one-half of the income forgone is replaced by the Government payment. A transfer program with a 75-percent tax rate would reduce further the cost of not working (or the gain from working) to 50 cents per hour. Other things being equal, a decrease in the cost of not working should lead to reductions in market work. However, the increase in tax rates not only reduces the effective cost of not working, but reduces income as well. For example, working 1,000 hours at a gross wage of $2 yields $500 when the tax rate is 75 percent as compared to $1,000 when the tax rate is 50 percent. On the one hand, the higher the tax rate is, the lower the cost of not working and, therefore, the less one will work. But on the other hand, the higher the benefit-loss rate, the lower the beneficiary's income opportunities. Therefore, the less able he is to afford to work less. Theoretically, we do not know which of these opposing effects is more important. Thus, higher benefit-loss rates can lead to either greater or lesser reductions in labor supply.

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[5] While all time spent in activities other than market work is called leisure in most of the economics literature, as most economists recognize, this is a misleading label. For while activities such as raising children, cooking, cleaning house, doing home repairs and going to school do not constitute market work, neither are they what is conventionally thought of as leisure.

[6] How much an individual actually works may also depend on the demand side of the market.

A transfer program with a positive guarantee and a positive tax rate both increases beneficiaries' income opportunities and reduces the cost of not working. Both changes lead to reductions in labor supply. A transfer program with a positive guarantee and a zero tax rate also reduces labor supply. Although the price of not working is unaffected by a zero benefit-loss rate, the individual's ability to afford not to work is increased by virtue of the increase in his income from the guarantee. Thus, static economic theory[7] unambiguously predicts that income transfer programs with zero or positive tax rates will lead to reductions in program beneficiaries' labor supply. But the theory says nothing about the magnitude of the effect.

Very minute and very large reductions in labor supply are equally consistent with the theory. How large the effects are, or will be, is an empirical question.

In contrast, a wage subsidy program—a program in which payments increase with hours of work—increases net wage rates and thereby increases the reward for working or, what is the same, the cost of not working. Just as a decrease in net wage rates simultaneously decreases income and decreases the cost of not working, an increase in net wage rates simultaneously increases income and increases the cost of not working. The increase in income leads to less labor supply while the increase in the price of not working leads to more labor supply. Which effect predominates cannot be ascertained theoretically. *Consequently, not only the magnitude but also the direction of the effect of wage subsidy programs on labor supply is an empirical question.* This runs counter to the popular notion that since a wage subsidy is paid only if one works, such a program must have a consistently positive effect on work effort.

Because most existing and proposed income transfer programs have positive guarantees and positive or zero tax rates, except where otherwise noted throughout the rest of the paper, the possibility of negative tax rates in earnings supplement or wage subsidy programs is ignored in the discussion of the effects of transfer programs. (The paper by Samuel Rea, Jr. in this volume deals directly with the labor supply effects of wage and earnings subsidies.) Transfer programs with positive guarantees and tax rates will be referred to as negative income tax (NIT) programs.

While economic theory provides no guide to the absolute magnitude of the reductions in work effort which would be induced by transfer programs, economic and sociological theory suggests that the effect will differ among demographic groups.

Consider, for example, prime-aged married males vis-a-vis prime-aged married females. Because of traditional differences in the roles that society expects husbands and wives to fulfill, the effects of a transfer program on their labor supply should differ. Husbands are expected to be breadwinners, to work full time; while wives are expected first of all to raise children and do housework and only second, if at all, to work. The roles are becoming less distinct—a phenomenon that may have partly resulted from—or led to—the current women's liberation movement. We no longer think it inappropriate for wives to work or for husbands to do housework. Even though these sex roles are blurring, the distinction still is an important one. One would expect a transfer program to lead to a larger reduction in the labor supply of wives than of husbands for two reasons. First, working less than full time or not at all is more socially acceptable for wives. Second, given current attitudes, wives' alternative use of their time—raising children and doing housework—is more valuable than husbands' alternative use of their time.

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[7] If changes in income change other variables which affect labor supply the result is more ambiguous. For example, increases in income could lead to better health or higher motivation, changes which in turn could actually lead to an increase in labor supply. For a more formal treatment of this dynamic case, see (10).

In this context, female heads of families are like wives for their nonmarket use of time is highly productive and raising children is a socially acceptable role. Thus, if income from nonemployment sources is sufficient, the probability of female heads working little or not at all is also expected to be high.

The effect of transfer programs on the labor supply of the aged should be larger than the effect on prime-aged husbands because not working—that is, retirement—is for the aged a socially acceptable role. Moreover, work is physically more difficult for many of the aged than for those younger. On the other hand, the aged's nonmarket use of time is not so productive as that of wives with young children to raise. Thus, it is difficult to say a priori whether the effect of income transfer payments on the labor supply of the aged is likely to be smaller or larger than the effect on prime-aged wives.

This brief review has suggested that: (1) transfer programs (with the possible exception of earning supplements or wage subsidies) will lead to reductions in the labor supply of program beneficiaries; (2) the magnitude of those reductions will vary among demographic groups; and (3) how large the reductions in the labor supply of any demographic group will be is an empirical question. In the next four sections the empirical evidence is presented and critically evaluated.

II. WORK RESPONSE OF PRIME-AGED MARRIED MALES

This section examines evidence from the New Jersey income maintenance experiment and from cross-section studies on the work response of prime-aged males. For three reasons this section is substantially longer than the sections on the other groups. First, some issues and problems common to the estimates for all groups are discussed in this section simply because it is the first one. Second, there is a much wider divergence in the literature about the work reaction of prime-aged males than of other groups. Third, prime-aged married males are of critical importance because: (*a*) they contribute such a large share of existing labor supply and (*b*) the most controversial feature of recent income transfer plans such as the family assistance program (FAP) is their proposed extension of coverage to poor families headed by able-bodied working males.

A. The New Jersey Income Maintenance Experiment

The advantages of experimentation are obvious. Experimentation allows us to dispense with the crucial assumption of cross-section analysis that individuals with different wage rates and different amounts of nonemployment income are, except for differences in other easily measured characteristics, identical. Because variations in maximum benefit amounts ("guarantees") and benefit-loss rates are experimentally controlled, we can have more confidence that variations in labor supply which are *associated with* variations in guarantees and tax rates are also *caused* by them.

There are also, however, disadvantages to experimentation. First, social experimentation is relatively costly. The New Jersey graduated work incentives experiment alone cost $7.9 million. Second, when human beings are the subject of investigation it is difficult to control all factors that affect behavior. Moreover, there are ethical limits to the amount of control that can be exerted. There are two very important problems in the New Jersey experiment which arose from an inability to control for factors that affect work behavior. After a brief description of the New Jersey experiment, these problems are discussed.

The New Jersey experiment began in August 1968, and lasted 3 years. The experiment was conducted in four New Jersey cities—Trenton, Paterson, Passaic, Jersey City—and also in Scranton, Pa. Only families whose normal income was below 1.50 times the Social Security Administration's poverty level were selected to participate. In order to focus on intact families, the sample was further limited to families which included at least one work-eligible male (aged 18–58, who was neither disabled nor a full-time student) plus at least one other family member. Families were assigned on a stratified random basis to either one of eight experimental groups or to a control group. Families assigned to the control group were not entitled to benefits from any of the experimental negative income tax plans. Each of the eight experimental groups were eligible for a different negative income tax program. Maximum benefits ranged from .50 to 1.25 percent times the poverty level, and tax rates ranged from 30 to 70 percent. (For a family of four in 1973 the guarantees would range from $2,000 to approximately $5,000.) It should be noted that none of the experimental plans had a work requirement.

Experimental and control families were interviewed every 3 months. These 12 quarterly questionnaires contained questions on the hours worked and earnings of all family members during the week previous to the interview and a host of other questions. The analysis reported here is based on these data.[8]

One problem with the experiment is that it lasted only 3 years. On the one hand, a temporary income guarantee increases lifetime incomes by a smaller amount than would a permanent guarantee, which suggests that the labor supply reductions which would be induced by a permanent guarantee are underestimated by the experiment. On the other hand, while a permanent program would reduce the price of leisure permanently, the experiment reduces it temporarily. That is, for experimental families leisure is on sale. This suggests that the experimental tax rate effects overestimate the labor supply reductions which would be induced by a permanent negative income tax program.

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[8] Experimental families also had to file income report forms every 4 weeks from which their payments were calculated. In addition, except for Paterson and Passaic, data on the welfare status of families were obtained from the local welfare departments. The latter data source was used to supplement the data available from the quarterly questionnaires in ascertaining welfare status.

A second problem is that during this period New Jersey and Pennsylvania had relatively generous welfare programs for which low income families with an able-bodied male head were eligible: Because control group families were already potentially eligible for a welfare program, the differences between the work efforts of the experimental and control groups for all eight plans are smaller than would be anticipated had the experiment been conducted in a State with a less generous welfare program. While, on the whole, the quantitative magnitude of the biases arising from these two problems appears to be rather small, the quantitative analyses upon which this conclusion is based are rather crude.[9] Finally, because the experiment was temporary and affected only a proportion of the potentially eligible population, the experimental results do not reflect any labor market or community changes in economic variables or in tastes for income vis-a-vis leisure that might result from a real, permanent program. Consequently, the experimental results, like those from even the best cross-section studies should be approached with some degree of skepticism.[10]

In table 2, differences between the hours worked and earnings of husbands in the experimental and the control groups are presented for the aggregate of all eight plans and separately for each of the eight plans. These differences are adjusted in a regression analysis for differences among sample observations in educational attainment and health status of the head, family size, ethnicity, location, and the family's welfare experience.[11] The sample consists of 741 husband-wife families who responded to more than half of the quarterly questionnaires. (The results reported here differ slightly from those reported in the HEW *Summary Report: New Jersey Graduated Work Incentive Experiment*, because the sample differs slightly and the results reported in the summary report focus on the middle 2 years of the experiment.) It is also important to note that the reported differences represent the *average* differences between all experimental and control participants. Because there are a priori reasons for believing that the labor supply reduction induced by a given negative income tax plan will, on average, be larger the lower a family's income or earnings capacity is, the average differences would have been smaller if families with incomes greater than 1.5 times the poverty level had been included in the experiment.[12] Conversely, the average differences would be expected to be larger if the analysis were restricted to the poorest families who participated in the experiment. Consequently, great care must be exercised in drawing inferences from the results reported below about the behavior of other population groups.

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[9] See Charles E. Metcalf (15) and Irwin Garfinkel (14) for more detailed and rigorous discussions of these problems and for quantitative estimates of the magnitude of the biases. Note in particular that while Garfinkel concludes that the biases are small if all experimentals are compared to controls or if experimentals in each plan are compared one at a time to controls, he argues that cross plan comparisons of guarantee and tax rate effects may be more seriously biased.

[10] An additional reason for skepticism at this point is that the results reported here are so fresh. As this paper was being prepared, analysis for the report to the Office of Economic Opportunity on the experiment was just being completed. But it is certain that those responsible for the preparation of the report will further analyze the data and that other researchers will reanalyze the data.

[11] The inclusion of the welfare status variables makes these differentials correspond to what Garfinkel (14) identifies as the "best" estimate of what the differentials would have been in the absence of welfare. See especially sec. 4 and app. II.

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[12] The lower an individual's or family's earnings capacity, the greater the probability that a given negative income tax budget constraint will dominate the pre-*NIT* equilibrium indifference curve. Consequently, for a given *NIT* plan the relationship between experimental-control labor supply differentials should be as depicted in fig. 1 below, where earnings capacity is measured along the horizontal axis and the absolute magnitude of the treatment-control labor supply differential is measured along the vertical axis. (Earnings capacity is assumed to always exceed zero so that the horizontal intercept is greater than zero.) At earnings capacity *A* the differential is *AB* while at *M*, the differential is zero. If families with earnings capacities between *A* and *M* were included in the experiment the average differential would be greater than zero and less than *AB*. The exact differential would depend upon how many of each kind of family was included.

FIGURE 1.—Experimental response and earnings capacity.

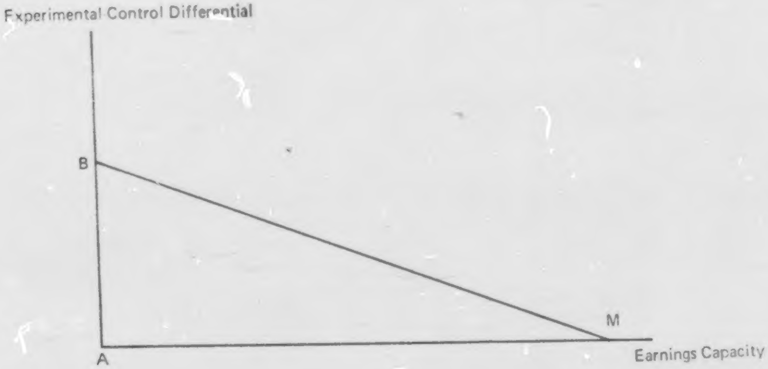

TABLE 2.—*Differences in hours worked and earnings of husbands in experimental and control groups in the New Jersey income maintenance experiment*

	Mean control group labor supply	All plans	Plans with guarantees (G) [2] as percent of poverty level and tax rates (TR) [1]							
			G=50 TR=30	G=50 TR=50	G=75 TR=30	G=75 TR=50	G=75 TR=70	G=100 TR=50	G=100 TR=75	G=125 TR=50
Hours worked per week	34.2	[3] −2.19	−2.96	2.30	−1.32	[3] −3.96	−1.75	[4] −5.36	−2.65	−[illegible].65
Earnings per week (in dollars)	97.6	.12	−11.66	10.03	4.99	.72	4.92	−10.61	−2.59	.62

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[1] Percentage rate by which earnings reduce benefits.

[2] For a family of 4 in 1971, the poverty level was about $4,000. Thus, the guarantees for a family of 4 in 1971 ranged from slightly under $2,000 to slightly under $5,000. For example, in the G=50, TR=30 plan, the guarantee was 50 percent of the poverty level (almost $2,000 to a family of 4 in 1971); $1 of earnings reduced benefits by 30 cents. Benefits would continue until earnings reached $6,667.

[3] Indicates significance at the .05 level.

[4] Indicates significance at the .01 level.

NOTE.—All figures shown in columns to the right of the column labeled "Mean control group labor supply" are differences from that column. Thus, minus signs indicate that the experimental group worked or earned less than the control group; positive numbers indicate that the experimental group worked or earned more than the control group. Except as designated by footnotes 3 and 4, none of the other differentials are significant at even the .10 level.

The first row in the second column indicates that the experimental groups worked about 2 hours less per week than the control group. Footnote 3 indicates that the probability of such a large difference occurring by chance if the real difference were zero is less than 5 percent. This amounts to about a 6-percent reduction in the labor supply of all husbands. Not only is this difference rather small but further examination of the data indicated that there was no difference of statistical significance between the percentage of experimental and control group husbands who did not work at all during any of the 3 years of the experiment. The evidence from the experiment, therefore, hardly supports the notion that if guaranteed an adequate income, the heads of poor families will permanently quit work en masse.

The most interesting aspect of table 2 is that while the difference in overall hours worked is negative, the earnings difference is slightly positive. This indicates that although on the whole husbands in the experimental group worked less than husbands in the control group, they earned more when they did work. There are at least two good alternative explanations for this finding.[13] First, because experimentals had to file income report forms every 4 weeks in addition to responding to the quarterly questionnaire, it is possible that they may have learned more rapidly than controls to report gross rather than net wages. To the extent that this learning phenomenon was responsible for the higher reported wage rates of experimentals, the difference should narrow over experimental time. For both Spanish-speaking and non-Spanish-speaking white husbands in the sample, this is precisely what happened. However, for blacks the wage rate differences actually grew. An alternative explanation, at least for the earnings differences among blacks, is that because experimental family members had the negative income tax payments to fall back on they could afford to be more selective about the jobs they took. That is, when they became unemployed they could take longer to search for better jobs, or they were more willing to quit their current jobs to look for better ones. In both cases, we would expect to find a higher proportion of the experimentals than the controls unemployed during any given time period. To the extent that the extra search paid off, the experimentals would have higher earnings per hour. These results suggest that income transfer programs may help reduce poverty not only by directly raising the income of poor families through transfers, but also indirectly, by enabling poor workers to be able to afford to search for better paying jobs and thereby increase their earnings. Whether or not the experimental negative income tax program actually had such an effect, even on blacks, is still not clear. Moreover, the experiment provides no information on whether more search would pay off if *all* poor workers engaged in more search rather than just the few who participated in the experiment. (On the other hand, the experiment cannot capture any market wage increases that would be induced in response to reductions in labor supply.)

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[13] A third explanation is that average experimental earnings would not decline as much as hours if experimental husbands with low wage rates reduced their labor supply more than those with higher wage rates. An examination of the data, however, revealed that this compositional effect was not a major factor in accounting for the wage rate increase.

The most puzzling aspect of table 2 is the clear absence of a distinct pattern of differences among the negative income tax plans in the experiment. The plan with the 100-percent poverty level guarantee and the 50-percent benefit-loss rate has the largest difference. Both the 100–70 and the 125–50 plans, with a higher benefit-loss rate and a higher guarantee, respectively, have substantially small differences; in fact, persons assigned to the latter plan—the most generous in the experiment—actually worked more than controls. On the other hand, the plan with the lowest guarantee and benefit-loss rate—the 50/30 plan—which we would expect to have one of the smallest differences, actually has the third largest. In most cases the differences between plans are not statistically significant. In addition, linear guarantee and tax rate coefficients were negative and positive, respectively, but statistically insignificant in all cases (that is, increases in the guarantee lead to small but insignificant decreases in labor supply, while increases in the tax rate lead to small increases in labor supply). While there may be other possible explanations for this puzzling absence of the expected pattern among plans,[14] perhaps the simplest explanation is that the sample size for each plan considered individually is too small.

Although 1,353 families were originally enrolled in the experiment, due to family breakups and sample attrition only 741 both were intact and had filled out more than half the quarterly questionnaires. Of these, 292 were assigned to the control group, leaving 29, 35, 63, 70, 51, 46, 54, and 101 respectively for each of the eight plans. Given the number of families in each plan, unusual or eccentric behavior on the part of a few individuals in the plans with fewer families in the sample could easily dominate the average labor supply values in those plans and, thereby, lead to relative distortions among the plans. Since the experimental group as a whole is so much larger than the number in any particular plan, it is more likely that cases of unusually low labor supply will be canceled out by cases of unusually high labor supply. Consequently the possibility of the difference between the means of *all* controls and *all* experimentals being dominated by a few unusual cases is reduced. For this reason, it seems likely that the difference between the control group and *all* experimental groups is more reliable than the differences between controls and experiments in any *particular* experimental negative income tax plan.

In summary then, because experimental data, like cross-sectional data, have deficiencies, the results derived from the New Jersey experiment must be viewed with caution. Estimates from the experiment are consistent with cross-section studies (see next section) which indicate that negative income tax plans would lead to some decrease in the labor supply of husband beneficiaries, but that the decrease would be small. *Moreover, even without a work test the experimental results indicate that almost all of the decrease will come in the form of working less, rather than quitting work entirely and "living off the dole."* Finally, while there was a slight decrease in the hours worked by husbands, it was more than offset by an increase in their wage rates so that the earnings of experimental husbands actually increased by a miniscule (and statistically insignificant) amount as a result of their participation in the experimental negative income tax plans.

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[14] Recall that in theory labor supply can increase with an increase in benefit-loss rates (decrease in effective wage rates) since this simultaneously reduces income and the price of leisure. Which effect predominates is impossible to specify a priori. However, holding income constant, a reduction in the price of leisure must lead to a reduction in work effort. The guarantee and tax rate estimates implicit in table 2 unfortunately imply the opposite and are therefore inconsistent with economic theory.

B. Cross-Sectional Studies

Since a transfer program of the negative income tax type (positive guarantee, positive benefit-loss rate) would simultaneously increase the amount of nonemployment income (NEY) available to beneficiaries and decrease their net wage rate, one way of estimating the potential labor supply effects of transfer programs is to examine the differences in labor supply of individuals with differing wage rates and differing amounts of nonemployment income. For example, the probable effect on work effort of a transfer program with a $3,000 guarantee can be estimated by measuring the average difference in labor supply associated with differences of $3,000 in nonemployment income between groups of individuals with identical wage rates and demographic characteristics. Similarly, the probable effect on work effort of a transfer program with a 50-percent benefit-loss rate can be estimated by measuring the average difference in work effort, between groups of individuals with identical amounts of nonemployment income and demographic characteristics, associated with differences of 100 percent in wage rates (that is, with one group's wage rate equal to half that of the other).

Numerous researchers have devoted a great deal of time and ingenuity to estimating the labor supply effects of transfer program by use of sample survey data. Before reviewing these studies, however, an inherent weakness of this kind of approach should be noted.

Individuals with different wage rates and different amounts of nonemployment income are likely to differ in other important ways that have not been measured in the survey but may affect work effort. For example, the nonpecuniary desirability of a job is likely to influence the amount of time an individual will work at it. If desirability varies positively with the wage rate—a fairly reasonable assumption—and if desirability is not controlled for, the use of differences in average labor supply at different wage rates to estimate work supply reductions will result in an overestimate. For while introduction of a negative income tax program with a 50-percent benefit-loss rate will reduce the effective wage rate of $2 per hour jobs to $1 an hour, it will not reduce the nonpecuniary desirability of $2 per hour jobs to the level of $1 per hour jobs.

Perhaps even more serious is the absence of a measure of personal ambition.[15] A greater-than-average amount of ambition may lead an individual to work harder than average, have a higher-than-average wage rate, and a higher-than-average amount of nonemployment income. In the absence of a variable to reflect differences in ambition, the differences in average labor supply corresponding to different wage rates will reflect not only the effect of wage rates on labor supply but the positive effect of ambition on both wage rates and labor supply. Consequently, the estimate of labor supply reductions based on the association between average labor supply and wage rates will be too high. The differences in average labor supply at different levels of nonemployment income, on the other hand, will reflect the positive effect of ambition on NEY and labor supply as well as the negative effect on NEY on labor supply. Consequently, the estimates of labor supply reductions based on the association between average labor supply and nonemployment income will be too low. This suggests that estimates of the effect of transfer programs on labor supply derived from even carefully done cross-section labor supply studies should be approached with a healthy dose of skepticism.

In this spirit, the results of a representative group of studies are reviewed below. Estimates which I have derived from these studies of percentage changes in labor supply per $1,000 guarantee in a transfer program and per 10 percentage points tax rate in a transfer program are presented below in table 3.[16] The most striking and disturbing aspect of table 3 is the wide divergence in the estimates. The Kalachek-Raines (21) study suggests a 5-percent reduction in labor supply per $1,000 guarantee, while the Garfinkel-Masters (15) study suggests only a $\frac{6}{10}$ of 1 percent reduction. The Kalachek-Raines study suggests a 5 percent *decrease* in labor supply per 10 percentage points increase in the rate at which benefits are cut, while the Hall (18) study suggests a 3 percent *increase* in labor supply. Estimates of the work reductions of male household head beneficiaries that would be induced by a transfer program with a $3,000 guarantee for a family of four and a tax rate of 50 percent range from only 3 percent to 40 percent.

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[15] To date only in the Greenberg-Kosters study has there been any attempt to control for the effects of ambition. Unfortunately, their measure of ambition may be nothing more than a second measure of NEY. (For a rigorous discussion of this subject see p. 356 especially footnote 13 in (8).) Thus, their results are inconclusive. Ashenfelter-Heckman in (4) use a predicted total income rather than a measured income measure. The problem with this kind of procedure, however, is that so few individuals have substantial amounts of NEY. Thus, differences in NEY are likely to be swamped by differences in earnings. The Office of Economic Opportunity Michigan Survey Research Center Income Dynamics Panel Study has questions which appear to measure economic ambition. Garfinkel and Masters are currently attempting to use this data source to ascertain if controlling for ambition makes a big difference in the NEY-labor supply relationship. At this point, all that can be said is that results from studies which have not controlled for ambition may lead to underestimates of a negative income tax labor supply reduction induced by a negative income tax.

[16] The estimates are presented in this form rather than the more conventional manner of reporting income and substitution elasticities so that their meaning will be more intelligible to the layman. This entails some sacrifice in rigor. For example, $1,000 in 1960 is not equivalent to $1,000 in 1967. Compared to the other sources of imprecision and error in the estimates, however, this source is minor.

TABLE 3.—*Percentage changes in the labor supply of prime-age married male beneficiaries in response to negative income tax programs*

Study	Data source-year [1]	Per $1,000 increase in maximum benefit payment (the "guarantee") [2]	Per 10 percentage points increase in the tax rate (benefit-loss rate) [3]
Ashenfelter-Heckman (3)	SMSA aggregates, 1960 Census.	−1. 0	0
Ashenfelter-Heckman (4)	SEO—1967	−3. 5	+1. 5
Bowen-Finegan (5)	SMSA aggregates, 1960 Census.	−3. 0	−. 3
Garfinkel-Masters (15)	SEO—1967	−. 6	−0. 2
Greenberg-Kosters (17)	SEO—1967	−5. 2	+5. 0
Hall (18)	SEO—1967	−6. 0	+3. 0
Hill (20)	SEO—1967	−14	+2. 0
Kalachek-Raines (21)	CPS—1967	−5. 3	−5. 0

[1] The SMSA aggregate studies are based on averages for the 100 largest standard metropolitan statistical areas taken from the 1960 Census. The current population survey (CPS) is an annual survey taken of a random sample of the U.S. population. The survey of economic opportunity (SEO) was specially designed to get better measures of the economic status of the poor, and in addition some groups of poor people were oversampled.

[2] The guarantee effects for the first 5 studies are calculated directly from the author's reported nonemployment income or other income coefficients. For the last 3 studies the guarantee effect is calculated by converting the total income elasticity, reported in table 9.1 in (8), to a linear slope coefficient. The labor supply figure used to convert the elasticity to the slope coefficient was 2,000 hours per year for all 3 studies while the income figures used were 6,000 for Kalachek-Raines, 5,000 for Hall, and 4,000 for Hill. The income figures are crude approximations of the means of the sample used by the authors. Where the authors ran separate labor supply equations for blacks and whites, a weighted average (.33 for blacks, .66 for whites) of their results was used.

[3] With the exception of those reported for the Bowen-Finegan and Garfinkel-Masters studies, all tax rate effects are calculated from a wage rate elasticity derived by adding the income and substitution elasticities reported in table 9.1 in (8). The tax rate effect for the Bowen-Finegan study is derived directly from their earnings coefficient evaluated at initial earnings of $4,000. The tax rate effect for the Garfinkel-Masters study is derived from preliminary unpublished results. Where the authors ran separate labor supply equations for blacks and whites, a weighted average (.33 for blacks, .66 for whites) of their results was used.

NOTE.—In all of the studies except Ashenfelter-Heckman (3) and Bowen-Finegan, labor supply is defined either as annual hours worked or annual hours in the labor force. In the Ashenfelter-Heckman and Bowen-Finegan study, labor supply is defined as the labor force participation rate in the SMSA in the week prior to the survey. The Garfinkel-Masters measure of labor supply does not include overtime or moonlighting. For a discussion of the implications of using different measures of labor supply see (13) and (15).

To calculate the effect of an NIT with a $3,000 guarantee and a 50-percent tax rate, multiply the figure in the guarantee column by 3 and the figure in the tax rate column by 5. Thus, the Garfinkel-Masters results indicate that such an NIT would lead to a 2.81 percent [3(−.6)+5(−.2)] reduction in the labor supply of male heads.

The most important differences in the results are due to alternative methods of resolving the problems of: (1) how to measure nonemployment income, (2) what sample to use, and (3) how to measure wage rates. How some methods of resolving these problems lead to biased estimates is discussed in the next three subsections.

1. THE CHOICE OF AN NEY MEASURE

While the measure of nonemployment income in most of the studies reported here is based primarily upon returns to assets (interest, dividends, rent), those by Hill (20) and Bowen-Finegan (5) include income transfers in their measure of nonemployment income. This imparts a negative bias to the NEY-labor supply relationship, a problem which is recognized by Bowen and Finegan. The problem is that transfer payments are frequently received precisely because the beneficiary *cannot* work. In these cases it is not the availability of transfer payments that led to reductions in labor supply, but rather the reduction in labor supply that led to the receipt of transfer payments. Beneficiaries in such cases would not have worked any more had there been no program. This is certainly the case for many public assistance (PA) beneficiaries, and for the vast majority of unemployment compensation (UC) beneficiaries. Moreover, the less a PA or UC beneficiary works the more benefits he will receive. Consequently the actual amount of PA and UC benefits received by individuals will be negatively related to how much individuals work even if the availability of these benefits has absolutely no effect on work effort.

Workmen's compensation (WC) and veterans' disability and pensions program (VB) benefits are similar to public assistance and unemployment compensation benefits. Most WC benefits are paid because of total temporary disabilities. As a result, the benefit amount will normally be inversely correlated with time spent working. The inclusion of WC benefits in NEY would lead to a spurious negative correlation between NEY and work effort. Veterans' disability payments like WC payments are likely to be the best available proxy for the severity of health limitation on work effort, while the veterans' pension program is an income-tested program, which makes it similar to the public assistance program. Thus, payments from either of these programs should not be counted in NEY.

The Hill measure of nonemployment income consists solely of these kinds of transfers plus pensions.[17] The Bowen-Finegan measure also includes interests, dividends and rents. Because they include these work-related transfers (PA, UC, WC, and VB benefits) in their measure of NEY, these studies cannot provide a reliable guide to the impact of transfer programs on labor supply.

2. CHOOSING THE APPROPRIATE SAMPLE

Two of the studies with the largest estimated effects of guarantees—Kalachek-Raines and Hill—excluded from their samples individuals with incomes above some arbitrary amount.[18] The rationale for excluding these individuals is that a negative income tax program would affect only low-income workers, and the reaction of low-income workers to changes in benefit-loss rates and nonemployment income might be different from that of high-income workers. Unfortunately, while the rationale for focusing on workers with low earnings potential is clear, the method of excluding all those whose total family income is greater than some amount insures that the negative relationship between NEY and labor supply will be too large. Total family income depends in part on how much the family head worked. Of all families with high NEY, only those with low earnings from employment of the family head will remain in the sample. These family heads will have worked less than the average head in the total population with the identical wage rate and NEY. Thus, a negative relationship between NEY and labor supply is achieved by sample construction.

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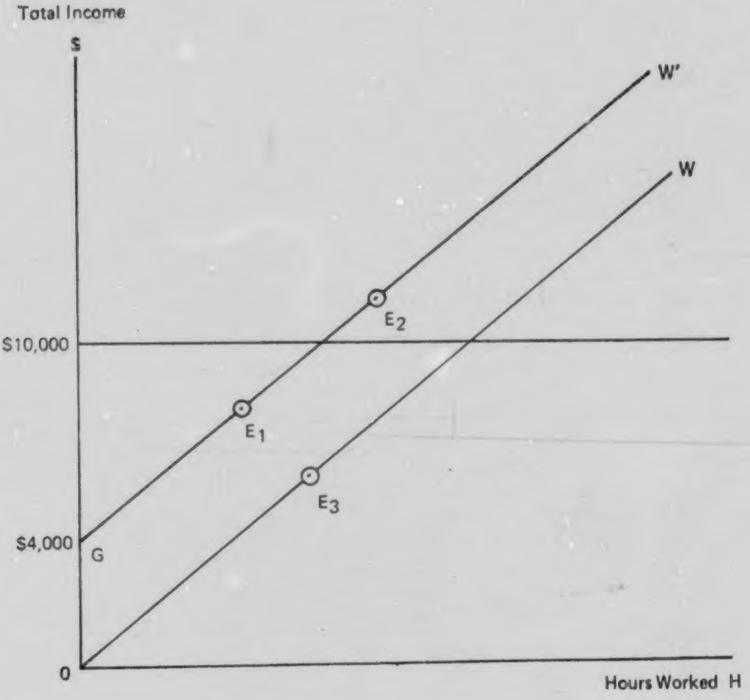
[17] Retirement pensions pose another kind of problem of holding tastes constant. Many individuals in the civil service, the military, and the private sector become eligible for retirement pensions well before the age of 65. To claim the pension, however, they must actually retire from their current job. If all individuals who were eligible did claim the benefits there would be no problem. But this is not the case. As of 1960, for example, 7.2 percent of civil service employees consisted of eligible retirees below the age of 65 who were not claiming their benefits (see (23) p. 87). One difference between claimants and nonclaimants who have identical alternative employment opportunities may be in their tastes for leisure vis-a-vis income. In other words, the pensions of claimants may represent, at least in part, a proxy for taste. The ideal procedure would be to devise a method to correctly describe the opportunity loci of both claimants and nonclaimants eligible for retirement. But it would be very difficult to identify the nonclaimant eligibles, and even if this could be done easily, the introduction of alternative budget constraints would complicate the estimation problem. Moreover, eligibility for pensions may in part reflect taste differences. Some occupations like the military and the civil services offer relatively generous pensions at an early age. Individuals who want to retire early are more likely to be attracted by such occupations.

[18] In the Kalachek-Raines study individuals in families with incomes greater than 2.5 times the poverty level were excluded, while in the Hill study individuals in families with incomes greater than the poverty level were excluded.

This point is illustrated with the aid of figure 2. Hours of work are measured from left to right on the horizontal axis and total income along the vertical axis. Imagine three individuals with identical wage rates, and assume two of them have nonemployment income of $4,000. Their income opportunities, or budget constraints, are given by the lines OW and OGW′ respectively. Let E_1 and E_2 denote the hours worked-income choices of the two individuals with nonemployment income and E_3 that of the person with no such income. By construction, there is no relationship between NEY and labor supply. However, if individuals with total incomes greater than $10,000 per year (the E_2 observation) are eliminated from the sample, the relationship between NEY and labor supply becomes very negative.

FIGURE 2.—Income cutoffs and nonemployment income coefficients.



Given their use of income cutoffs, it is not surprising that the Kalachek-Raines and Hill studies get such large negative guarantee effects. Because this procedure of selecting a sample insures biased NEY-labor supply relationships, their estimates are not reliable. If individuals with high earnings capacity rather than high actual income are excluded from the sample, however, because earnings capacity does not depend on actual hours worked, the problem of building a negative NEY-labor supply relationship into the sample can be avoided. The Garfinkel-Masters (14) estimates *reported in table 3 above are* derived from a sample which includes only workers with low earnings capacities.

3. THE WAGE RATE MEASURE

Just as an increase in Federal income tax rates reduces the effective wage rate, so an increase in a transfer program's tax rate reduces the beneficiary's net gain from work. Such reductions in the reward for work will, other things being equal, lead to reductions in work effort. But, it is also possible, as described earlier, for higher tax rates to lead to an *increase* in work effort because of reductions in income. That is, there can be a negative relationship between the net wage rate and work effort. The studies reviewed here found both effects—higher tax rates (lower net wage rates) leading to both increased and decreased work effort. There are technical problems, however, with those studies which show large effects in either direction (see table 1).

Greenberg-Kosters and Hill obtain a negative relationship between wage rates and labor supply at least in part because of the way in which they measure labor supply and wage rates. Their measure of labor supply is hours worked in the previous year. Unfortunately the only comprehensive measure of hours worked in the data base used—the Survey of Economic Opportunity—is hours worked during the previous week. To derive hours worked for the year, they multiply hours last week times weeks last year. Their wage rate is derived by dividing normal weekly earnings by actual hours worked during the previous week. As a consequence, individuals who worked more than their normal hours during the week previous to the survey will appear to have high labor supply and low wage rates. Individuals who worked less than their normal hours will appear to have low labor supply and high wage rates. Thus a negative wage rate-labor supply relationship is built into their data simply as a consequence of their definitions of labor supply and wage rates.

Hall's wage rate-labor supply relationship has a similar built-in negative bias. His measure of labor supply is last year's earnings divided by a potential wage rate measure. If the potential wage rate is too high (low) labor supply will be too low (high).

On the other hand, the Kalachek-Raines estimate may be biased in the other direction. Because the authors believe that reported wage rates contained substantial measurement error,[19] they assigned individuals a potential wage rate based upon their years of education, age, race, location, and other characteristics. The potential wage rate variable, however, may measure not only the effect of differences in wage rates on labor supply, but also the independent effects of differences in the other variables on labor supply. Consider, for example, years of schooling. Education not only increases an individual's productivity, but it may also change his tastes and affect the nonpecuniary aspects of jobs which an individual can get. It seems reasonable to assume that those with more education are most likely to have been socialized into a greater desire to work and that the more education an individual has the more pleasant his job is likely to be. Even more important, the number of years of education that an individual has completed may be the best proxy that we have for his ambition. That is, it is reasonable to assume that, on the average, individuals who drop out of school earlier than average will not only be less bright than average but less ambitious as well. Because Kalachek-Raines control for all the variables which they used to assign the potential wage except for education, their potential wage rate amounts to nothing more than an education variable scaled in wage rate units.

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[19] In the 1967 SEO, used by Garfinkel-Masters, the reported hourly wage rate is equal to normal weekly earnings divided by actual hours worked in the previous week. The mixture of *normal* earnings with *actual* hours leads to the possibility of severe measurement error for those who worked abnormal hours in the survey week. The CPS used by Kalachek-Raines has no direct measure of the wage rate.

Because the Bowen-Finegan study is based on aggregate rather than individual data, they avoid the problem of choosing between what may be a poorly measured wage rate and a potential wage rate variable. They estimate the relationships between the weekly labor force participation rate in a standard metropolitan statistical area (SMSA) and the average earnings of full-time workers in that SMSA. While aggregate data in general are often subject to the same problems as individual data,[20] at the very least their wage rate results constitute an independent piece of evidence which suggests that husbands' labor supply will decrease as benefit-loss rates increase (reducing the effective wage rate), but the decrease will be relatively small.

On the other hand, the decrease in male hours worked as wage rates have increased over time suggests that husbands' labor supply will increase as benefit-loss rates increase. At this point, whether the effect of increases in tax rates will be to increase or decrease male labor supply is not clear. Given the problems with studies that get large effects either way, however, it is probable that whether the effect is positive or negative, it is not likely to be large.

4. SUMMARY EVALUATION OF CROSS-SECTION EVIDENCE

The discussion in the three previous subsections indicates that there are very good reasons for discounting the highest estimates presented in table 3. Three of the other studies which indicate that high maximum payments (large guarantees) have large impacts on work effort—those by Hall and the second one by Ashenfelter-Heckman—used a slightly different methodology in which the guarantee effect is derived in large part from the wage rate effect. Note that in these cases the large positive benefit-loss rate effects cancel the large negative guarantee effects so that the net effect on work effort of a negative income tax program similar to the family assistance plan would be small. Estimates derived from the best cross-sectional studies indicate that work reduction of male heads induced by a negative income tax with a guarantee and tax rate similar to those proposed in the family assistance plan ($2,500 and 50 percent) would be small—from about 1 to 6 percent. These figures are consistent with those from the New Jersey income maintenance experiment. As argued above, however, there are also very good reasons for being skeptical of even the best of the cross-section studies.

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[20] Average earnings of full-time workers, for example may be a poor proxy for the earnings or potential earnings of marginal workers. If the relationship of the average to marginal earnings or wage rates varies substantially across SMSA's, the average earnings variable would contain measurement error and the earnings coefficient would be biased toward zero. Moreover, with aggregative data there is the danger that labor supply affects the wage rate rather than the wage rate affecting labor supply. This would also lead to a negative bias in the earnings coefficient. On the other hand, differences in wage rates may reflect disequilibriums in the labor market. High wage rates may reflect excess demand and low wage rates excess supply. This would impart a positive bias to the earnings coefficient.

III. WORK RESPONSE OF PRIME-AGED MARRIED WOMEN

Empirical studies on the labor supply of married women uniformly indicate that married women whose families become eligible for negative income tax benefits will work substantially less. Evidence from the New Jersey experiment indicates rather large reductions in work effort by wives. Using cross-sectional data, exact estimates vary from one study to another, but even the lowest estimates are substantial.

A. The New Jersey Income Maintenance Experiment

In table 4 the labor supply differences between wives in the experimental and control groups are presented for the aggregate of all eight plans and separately for each of the eight plans. The sample, and the dependent and independent variables are all identical to those used for husbands. More important, the data are subject to the same kinds of limitations.

The differences in the first column indicate that wives in the experimental group worked 0.6 of an hour less and earned $1 less per week than the wives in the control group. (The standard errors of these estimates are so large, however, that none is significantly different from zero.) While the absolute magnitude of these figures is small, the average labor supply values of wives in the control groups is also exceedingly small—only 30 percent of the wives ever worked and for all the wives the mean hours worked per week was about 4—so that the relative reduction in the labor supply of wives implied by the differences is fairly large. Wives in experimental families worked approximately 15 percent fewer hours per week than wives in control families and almost all of the difference is due to a lower employment rate for the former. These results are consistent with cross-sectional studies which indicate that negative income tax programs would induce substantial reductions in the labor supply of wives.

TABLE 4.—*Differences in hours worked and earnings of wives in the experimental and control groups in the New Jersey income maintenance experiment*

	Mean control group labor supply	All plans	Plans with guarantees (G) [2] as percent of poverty level, and tax rates (TR) [1]							
			G=50 TR=30	G=50 TR=50	G=75 TR=30	G=75 TR=50	G=75 TR=70	G=100 TR=50	G=100 TR=70	G=125 TR=50
Hours worked per week	4.2	−.63	−2.32	2.09	−1.58	−.86	.63	.44	.04	−1.52
Earnings per week (in dollars)	8.9	−1.09	−4.26	6.21	−2.39	−1.59	2.05	1.82	−.54	−3.96

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[1] Percentage rate by which earnings reduce benefits.

[2] For a family of 4 in 1971, the poverty level was about $4,000. Thus, the guarantees for a family of 4 in 1971 ranged from slightly under $2,000 to slightly under $5,000. For example, in the G=50, TR=30 plan, the guarantee was 50 percent of the poverty level (almost $2,000 to a family of 4 in 1971); $1 of earnings reduced benefits by 30 cents. Benefits would continue until earnings reached $6,667.

NOTE.—All figures shown in columns to the right of the column labeled "Mean control group labor supply" are differences from that column. Thus, minus signs indicate that the experimental group worked or earned less than the control group; positive numbers indicate that the experimental group worked or earned more than the control group. None of the differentials are significant at even the .10 level.

While wives in the experimental group worked about 15 percent less than wives in the control group, they earned only 12 percent less. Thus, as with husbands, wives in the experimental groups who worked earned more per hour than wives in the control group, suggesting the possibility of some additional indirect positive effects of a negative income tax program.

For wives as for husbands, we find no consistent pattern in the work responses induced by different plans. For example, the plan with the lowest guarantee and tax rate—the 50/30 plan—has the largest negative impact on hours worked, although we would expect it to have one of the smallest impacts. Further, the plan with the highest guarantee has only the fourth largest impact on wives' work. Because we expect the labor supply of wives to be more responsive to negative income tax programs than that of husbands, the perverse nature of differences in work effort of wives among plans is especially disturbing.[21]

Again, the most plausible explanation lies in small sample size, a problem that is exaggerated for wives because such a large percentage of wives in the general population do not work to begin with (only 50 percent work), and, because of the sample selection criteria, an even smaller proportion of wives (only 30 percent) in the New Jersey sample worked. Because only families with incomes equal to or less than 150 percent of the poverty level were eligible to participate in the sample, given the husband's earnings, a family was far more likely to be eligible for the experiment if the wife did not work. Given the small sample size in each negative income tax plan and the even smaller number of wives in each negative income tax plan who ever worked, it would not be too surprising for the results in a few plans to be dominated by the idiosyncratic behavior of one or two wives in those plans. Consequently, it seems likely that the differences for experimentals in *all* plans vis-a-vis controls are also more reliable for wives than the differences between experimentals in *any one* plan and controls.

B. Cross-Sectional Studies

Estimates derived from five different studies of percentage changes in labor supply per $1,000 guarantee and per 10 percentage points in the benefit-loss rate of a negative income tax plan are presented below in table 5. While there are some differences among the studies, all suggest that the effect of a negative income tax program on the labor supply of beneficiary wives would be large.

The estimated reductions per $1,000 of guarantee range from 4 to 30 percent, while the estimated reductions per 10 percentage points of tax rate range from 4 to 10 percent. These estimates generally are substantially larger than the estimates of the percentage reduction in husbands' labor supply. And, for studies done by the same authors using the same data and methodology for both groups, the estimates are much larger.[22] These results are consistent with the a priori expectations discussed in section I.

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[21] Linear guarantee and tax rate coefficients are negative and positive respectively and in few instances are statistically significant. These coefficients imply that, holding income constant, a decrease in the price of not working would lead to an increase in labor supply, a result which is not only inconsistent with economic theory but also inconsistent with all other empirical studies of wives' labor supply.

TABLE 5.—*Percentage changes in the labor supply of married women beneficiaries in response to negative income tax programs*

Study	Data source [1]	Per $1,000 increase in guarantee [2]	Per 10 percentage points increase in tax rate (i.e., a reduction in the effective wage rate) [3]
Ashenfelter-Heckman (3)	SMSA aggregates in Census 1960.	−26	−9
Cain (7)	SMSA aggregates in Census 1950 and 1960.	−8 to −12	−5 to −10
Bowen-Finegan (5)	SMSA aggregates in Census 1960.	−6 to −30	−5
Garfinkel-Masters (15)	SEO—1967	−4	−4
Hall (18)	SEO—1967	−10	−4

[1] The SMSA aggregate studies are based on averages for the 100 largest standard metropolitan statistical areas taken from the 1960 Census. The current population survey (CPS) is an annual survey taken of a random sample of the U.S. population. The survey of economic opportunity (SEO) was specially designed to get better measures of the economic status of the poor, and in addition some groups of poor people were oversampled.

[2] The guarantee effects are calculated directly from the income coefficients reported in the studies. In the Bowen-Finegan study the husband's income coefficient is used for the lower bound and the NEY coefficient is used for the upper bound. The estimates for Cain are taken from his summary chapter.

[3] The tax rate effects for the Ashenfelter-Heckman and Hall studies are calculated from a wage rate elasticity derived by adding the income and substitution elasticities reported in table 9.1 in (8). The tax rate effect for the Bowen-Finegan study is derived directly from their earnings coefficient evaluated at initial earnings of $4,000. The tax rate effect for the Garfinkel-Masters study is derived from preliminary unpublished results.

NOTE.—The Garfinkel-Masters and Hall studies define labor supply as annual hours worked. The other studies define labor supply in terms of labor force participation rates during the week prior to the census. The latter is 1 part of the former.

To calculate the effect of an NIT with a $3,000-guarantee and a 50-percent tax rate multiply the figure in the guarantee column by 3 and the figure in the tax rate column by 5. Thus, the Garfinkel-Masters results indicate that such an NIT would lead to a 32-percent $[3(-4)+5(-4)=32]$ reduction in the labor supply of beneficiary wives.

For wives, the estimates of the effect of the guaranteed payment level are derived from differences in labor supply associated with differences in husbands' earnings (or total family income less wife's earnings) as frequently as they are derived from the association between differences in nonemployment income (NEY) and differences in labor supply. The two largest estimates (26 and 30 percent)—by Ashenfelter-Heckman and Bowen-Finegan are based upon the same aggregate SMSA measure of nonemployment income. Bowen and Finegan did not believe their own results and relied instead upon their estimates derived from husbands' earnings. (Bowen and Finegan rejected the NEY measure because it included public assistance—which as noted above would lead to biased estimates.) In most other studies the guarantee estimates derived from husbands' earnings and NEY are rather similar. While there are still nontrivial differences in the other estimates, there does not appear to be any criteria by which one or another estimate could be judged to be clearly superior. Consequently, even the range of reasonable guarantee estimates for wives is fairly substantial.

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[22] Only the estimate derived from the Kalachek-Raines study is comparable in magnitude to the estimates for the wives. For reasons discussed above, however, their estimates are clearly too large.

The estimates of the effects of the benefit-loss rate may be subject to a greater upward bias. Because more than one-half of the married women do not work, it is necessary, when using individual data, to devise a potential wage rate to estimate the effect of changes in net wage rates on labor supply. But using a potential wage rate for women may result in greater biases than using one for men. Education and occupation are normally the best predictors of the wage rate. But, particularly for women, education and occupation are likely to be excellent proxies for individual preferences for work. That is, women who plan to work are likely to get more education than those who do not have such plans and they are also likely to end up working more. (In addition, education beyond high school may increase preferences for work.) Similarly, certain occupations such as teaching require a commitment to full-time work.

The wage rate-labor supply relationship, therefore, will be positive not only because higher wages lead married women to supply more labor, but also because higher potential wage rates are a proxy for greater preferences for work. As a consequence, the estimates of the negative effects of tax rates on the labor supply of wives may be too high.[23]

One way to avoid this difficulty is to use aggregate (SMSA) data, where the wage rate is measured by the average earnings of women who work full time. While results based on this approach are consistent with those based on individual data, it is possible that the ease of finding and holding jobs plus the nonwage attractiveness of jobs are all positively correlated with the wage rate because employers can be expected to respond to excess demand (which could persist for a fairly long period of time) by raising market wage rates and increasing the nonwage attractiveness of jobs.[24] Thus, these aggregate results may also contain an upward bias.

A final approach is to see whether cross-section estimates are consistent with the long-term increase in labor force participation rates for married women, which has occurred along with a marked rise in the real wages available to women and in husbands' real earnings. In this regard, Bowen-Finegan and Cain concluded that the income and wage effects derived from cross-sectional analysis are consistent with some increase in the labor force participation of wives but not with so big an increase as has occurred. At first glance, this conclusion might suggest that the cross-section estimates are too low rather than too high. In addition to wage rates and husbands' income, however, other factors affecting the labor supply of wives have changed. Perhaps the most important change is that it has become increasingly socially acceptable for married women—even those with young children—to work. Thus, once again, it is not possible to draw any definitive conclusions concerning the amount of bias in the cross-section estimates. However, it does not seem too likely that *each* of these biases would be very large. Thus, it is doubtful that there is a large bias in most of the estimates for the effect of either the tax rate or the guarantee and, hence, the cross-section studies lead to the prediction of fairly substantial reduction in labor force participation of married women participating in income transfer programs.

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[23] As Hall and others have pointed out, home productivity may be positively related to a wife's market wage. To the extent that this relationship is important, the individual cross-section results might underestimate the tax-rate effect of a negative income tax. However, it is probable that the tastes problem is considerably more important.

[24] If markets are not competitive—for example, with a higher wage rate established due to monopoly power of unions or government minimum wage legislation—then the average market wage is a poor measure of the wage a marginal entrant to the labor force can achieve and the aggregate results may be quite misleading. While Bowen-Finegan present evidence showing that this may be an important problem for young males, it probably is not too important for women since most women are employed in reasonably competitive occupations where most wages exceed the legal minimum.

IV. WORK RESPONSE OF FEMALE HEADS OF HOUSEHOLDS

Evidence from two kinds of cross-sectional data indicates that the labor force participation rates of female heads of households are about as sensitive to economic factors as are those of married women. As with married women, while there are some large differences in the estimates of the labor force response of female family heads to income transfer programs, even the smallest ones are quite substantial.

A. Cross-Section Studies

Results from the Bowen-Finegan (5) study suggest that divorced, separated, or married women with husband absent decrease their labor supply by approximately 25 to 30 percent per $1,000 in nonemployment income (NEY) and from 0 to 17 percent per 10-percent reduction in their net wage rates (which is equivalent to increases in the benefit-loss rate). Preliminary estimates by Garfinkel and Masters indicate that female heads of families work about 10 percent less per $1,000 of NEY and about 6 percent less per 10-percent reduction in their net wage rates. However, the NEY estimate in the former study is too large because the NEY measure includes public assistance (see the discussion on p. 15).

B. Studies of the AFDC Program

The Garfinkel-Masters estimates for non-AFDC mothers are somewhat larger than estimates by Garfinkel and Orr of the effect of differences in State AFDC benefit levels (guarantees) and tax rates on the employment rates of AFDC mothers.[25] Garfinkel-Orr found that on average the employment rates of AFDC mothers decreased by about 4.5 percent as the annual guarantee increased by $1,000 and that a 10-percent increase in the benefit-loss rate led to about a 2-percent decrease in employment rates. However, they also found that a $1,000 increase in the guarantee had a larger effect the smaller the initial guarantee.[26] An increase from a $500 guarantee to a $1,500 guarantee, for example, led to a decrease in employment rates of about 14 percent. Hausman, who examined differences between the employment rates of AFDC mothers who resided in Mississippi,

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[25] A study by Gary Louis Appel (2) indicates that the 1967 Social Security Amendments which reduced the tax rates on earnings in the AFDC program did lead to increases in AFDC employment rates in Michigan. While the decrease in tax rates led to an increase in employment rates it also led to an increase in the number of AFDC beneficiaries by increasing the break-even level of income.

[26] This result was not reported in (16).

Alabama, and Kentucky, found that the effects of differences in guarantees and tax rates were much larger. If extrapolated, his estimates suggest that a $1,000 increase in the guarantee would lead to a 40-percent decrease in employment rates, while a 10-percent increase in the benefit-loss rate would lead to a 4-percent decrease in employment rates (19).

Both estimates, however, could be too high if the guarantee is serving as a proxy for how much administrative pressure States exert on AFDC mothers to work. States with higher AFDC payment levels may exert less pressure on beneficiaries to work. If this is true, in the absence of a measure of variations in administrative compulsion to work, the guarantee variable will reflect not only the negative effects of higher guarantees on labor supply but also the negative effect of less administrative compulsion to work. This problem may be particularly serious when only the labor force participation rates of AFDC mothers in Mississippi, Alabama, and Kentucky are being compared because the first two States, which have substantially lower guarantees than Kentucky, also are reputed to engage in more administrative compulsion. Moreover, they both had work requirements while Kentucky did not.[27]

On the other hand, there is at least one factor which acts to depress both estimates. The higher the guarantee, other things being equal, the more one can earn and still remain an AFDC beneficiary. Because the sample in both studies consists only of AFDC beneficiaries, a positive relationship between the guarantee and the employment rate may be built into the sample and will offset the negative relationship of interest.

Given the available data, it is not possible to assess the relative importance of these potential biases which work in opposite directions. What is clear, however, is that the empirical evidence uniformly suggests that the labor supply of female heads of households, like that of wives, is highly responsive to both the amounts of income that they can get from sources other than employment and to the net monetary rewards that they can get from working.

V. WORK RESPONSE OF OLDER MEN

There are some unique problems to estimating how sensitive the labor supply of retirement age individuals is to income and net wage rate changes. Before proceeding to an examination of the empirical evidence, a brief discussion of these problems will be useful.

Individuals age 65–72 present particular problems because their eligibility for old age insurance (OAI) benefits is complicated by the retirement test. Under the retirement test, if earnings exceed a given amount, OAI benefits are reduced. Consequently, other things being equal, there is bound to be a negative relationship between the level of OAI benefits and labor supply for individuals age 65–71. Thus, if OAI benefits are included, the relationship between nonemployment income (NEY) and labor supply will be negative not solely because the existence of NEY led to reduced labor supply, but also because reduced labor supply led to higher NEY in the form of OAI benefits.[28]

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[27] Garfinkel and Orr found that other things being equal, the employment rate of AFDC mothers was 13 percent higher in States with work requirements than in States without work requirements.

[28] Hall in (18) includes social security in his measure of NEY. For this reason his estimates of the effect of NEY on the labor supply of males older than age 60 are of little use.

But because OAI benefits are so large relative to other sources of NEY and because most of the individuals in this age group get OAI benefits, to simply ignore their existence is untenable. For the 62–64 age group, this problem is compounded by the fact that OAI benefits are available on a reduced benefit basis. Individuals who elect to take reduced benefits are likely to be less healthy or have greater preferences for leisure than individuals who wait until age 65.

Thus, there are problems with estimating the effects of the guarantee and the tax rate for individuals age 61–71. Consequently, in this section I focus on results from studies of males age 55–61 in 1967 and age 55–64 in 1960 when males were not eligible for reduced social security payments. In addition, some results for males age 72 or over are reported.

A. Cross-Section Studies

Estimates by Bowen and Finegan indicate that the weekly labor force participation rate of males age 55–64 declines by about 10-percent per $1,000 in NEY and by about 1 percent per 10-percent decrease in net wage rates (equivalent to an increase in benefit-loss rates). Both of these estimates are considerably higher than their estimates for prime-aged married males. As in the case with prime-aged males, however, the NEY measure includes transfer payments so that the estimate of the effect of the guarantee is too large.

Garfinkel and Masters estimate that married males age 55–61 work 5 percent less per $1,000 NEY and 0.4 percent less per 10-percent decrease in their net wage rates. Their estimates for males age 72 or over indicate that members of this group work 10 percent less per $1,000 NEY and about 1 percent less per 10-percent decrease in their net wage rates. Thus the labor supply of older males is more sensitive than that of prime aged males to both increases in NEY and decreases in net wage rates.

On the whole these results are consistent with the hypothesis in the first section that the labor supply of older workers will be more sensitive than the labor supply of prime-aged married males. The results also suggest that the labor supply of older men may not be quite so sensitive as that of married women or female heads of households.

B. The Work Incentive Effects of Social Security Retirement Benefits (OAI)

The OAI program both increases income by providing retirement benefits and reduces the cost of not working by reducing (that is, taxing) those benefits as earnings increase above a certain amount. To date no serious attempt has been made to estimate the income or guarantee effect of the OAI program.[29] In the absence of such studies, the estimated effects for 55–61 year olds and those over age 72 serve as a good proxy. Several studies, however, have attempted to estimate what effect the earnings test has had on labor supply. The best two are by Bowen and Finegan and by Vroman (27).

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[29] Lowell Gallaway in (12) claims to have estimated an income effect by estimating the relationship of the ratio of average OAI benefits to average earnings in a State to the labor force participation rates of the aged in that State. But the OAI benefit-earnings ratio may be measuring tax rate rather than income or guarantee effects.

Bowen and Finegan show that when education, income, and other demographic characteristics are controlled for, the labor force participation rates of older men in 1960 declined precipitously at age 65, then declined steadily until age 72 when they actually increased and then began declining again. In 1960, men were eligible for OAI payments at age 65, and the payments were subject to the retirement test (that is, tax) until age 72. They attribute the jump in labor force participation rates at age 72 to the removal of the retirement test at age 72.[30]

Vroman studied the effect of the 1965 Social Security Amendments which increased the earnings range with a zero marginal tax rate from $1,200 to $1,500. He discovered that in response to this change, about 10 percent of both male and female OAI beneficiaries increased their earnings from just below $1,200 to just below $1,500. Because no comparable change took place in the years immediately prior to or subsequent to 1965, it is difficult to attribute the change to anything other than the change in the law in 1965.

In short, the labor supply of older men is sensitive to guarantees and tax rates. And, the OAI system has undoubtedly enabled older men to afford to work less by providing retirement income and has further discouraged work by reducing these benefits via the earnings test.

VI. OTHER PROGRAM STUDIES OF THE EFFECT OF TRANSFERS ON WORK EFFORT

Several studies have been done on the work effort of beneficiaries of State general assistance programs and State unemployment insurance programs. General assistance programs are cash programs based on current family needs. These programs are funded and operated entirely by State and local governments. Because the beneficiaries of these programs are members of a variety of demographic groups and because these studies made no attempt to isolate the effects of the program on any particular demographic group, discussion of these studies is relegated to this last section.

Unfortunately, despite the claims of their authors, studies by Brehm-Saving (6), Albin-Stein (1), and Kasper (22) of the general assistance programs (GAP) tell us nothing by themselves about the impact of these income transfer programs on work effort. These studies estimated the relationship between GAP benefit levels and the proportion of a State's population receiving GAP payments. But other things being equal, the higher the benefit level is, the larger the proportion of a State's population that is eligible for GAP payments will be. Thus, GAP benefit levels and beneficiary rates will be positively correlated even if benefit levels have no effect whatsoever on the labor supply decisions of actual or potential beneficiaries.

In contrast, two studies by Raymond Munts (26) and Gene Chapin (9) on the unemployment insurance (UI) program do provide some useful information on the UI systems' work incentive effects. Munts uses UI claims data from Wisconsin. In Wisconsin, as in other States, reduced benefits are paid to the partially unemployed, but in Wisconsin (and a few other States) there is a set of extreme implicit marginal tax rates in the partial benefits schedule. If the worker earns less than one-half as much as his weekly UI benefit amount, his UI payment is equal to the full benefit amount. But his UI payment is reduced by one-half if the individual earns at least one-half but less than his full weekly UI benefit amount. And if the individual's earnings are equal to or greater than his weekly benefit amount, he gets no UI payment. As a result of this peculiar set of marginal tax rates, workers have an incentive to adjust their part-time labor supply to earning just less than half their benefit amount, or if they must work more, then up to just less than the full amount of their weekly benefit amount. Munts' examination of the distribution of earnings of those filing for partial UI benefits indicated that, indeed, the claims were heavily bunched at these two points, particularly the former.[31] These findings indicate that many workers are aware of the economic incentives in Wisconsin's UI system, are able to adjust their work effort to take advantage of the system, and do—in fact—adjust their labor supply in response to the system's peculiar incentives. Unfortunately, the Munts study was not designed to provide any quantitative estimates of the magnitude of the work reductions induced by the UI system. Nor is it clear that these findings on the effects of extreme disincentives can be generalized to the effects of less extreme work disincentives.

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[30] Gallaway in (12) arrives at a similar conclusion.

The Chapin study also provides evidence that the implicit tax rates in the UI system affect the work effort of UI beneficiaries. Chapin estimates the relationship between the State's average duration of unemployment insurance claims and the ratio of average weekly UI payments to average weekly earnings in the State. The higher benefits are relative to earnings, the higher the implicit tax is on working and the lower the monetary reward is for returning to work.[32] Chapin estimates that a 10-percent increase in benefits relative to wages leads to a 1.3-percent increase in UI beneficiaries' duration of unemployment.

A problem with Chapin's study is that he makes no attempt to control for differences in State eligibility provisions.[33] However, given the evidence in the Munts study, it is difficult to doubt that the UI system does lead to some reduction in labor supply.

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[31] The probability of finding this kind of distribution by chance was less than one in a thousand.

[32] The income effect of the UI system should be minor for most workers because the lifetime additions to their income from UI payments will be miniscule. For workers who regularly become unemployed, however, this may not be the case. Workers in seasonal industries who are eligible for UI payments, for example, might get substantial increases to their lifetime incomes from the UI system. To the extent that such regularly unemployed workers play an important role in accounting for the variations across States in unemployment duration, Chapin's estimates may include income as well as tax rate effects.

[33] Numerous States, for example, have provisions in their UI laws which are designed to make seasonal workers ineligible for UI benefits, while other States have no such provisions. If the average unemployment duration of seasonal workers is higher than that of nonseasonal workers and if States with lower benefit earnings ratios tend to exclude seasonal workers from coverage while those with higher benefit earnings ratios do not, the benefit earnings ratio variable may be reflecting the influence of eligibility laws on the duration of unemployment insurance claims in addition to the influence of the implicit tax rate in the UI system on the actual duration of unemployment.

VII. SUMMARY AND CONCLUSION

Empirical studies based on cross-sectional sample survey data, experimental data, and income transfer program data confirm the a priori prediction derived from economic theory that income transfer programs will induce program beneficiaries to work less. Furthermore, on the whole, the studies confirm the hypothesis that the labor supply of prime-aged husbands will be affected much less by transfer payments than the labor supply of wives, female heads of households and older men. While almost all studies of the labor supply of wives, female family heads, and older men indicate that transfer payments will lead (or have led) to fairly sizable reductions in their labor supply, most of the more reliable studies of the labor supply of prime-aged husbands indicate that transfer programs would lead to relatively small reductions in their work effort. But there are problems with even the best labor supply studies which preclude precise estimates of the effects of transfer programs on any group.

Even though new Government income transfer programs might induce some substantial work reductions among certain groups of beneficiaries, such as wives, it is important to bear in mind that the effects on the national economy will be very small. This is the case because the families that would be eligible for most proposed income payments constitute a relatively small proportion of the existing work force and their output represents an even smaller proportion of total output.

Finally, while the empirical evidence reviewed in this paper makes it clear that one cost of transfer programs is a reduction in labor supply, no implications for transfer policy follow. All programs have costs. This paper has discussed only one of the important costs of transfer programs.

No attempt has been made to evaluate the importance of this cost. To do so requires value judgments. Nor has any attempt been made to weigh the relative importance of other costs and benefits of income transfer programs. Given the widespread concern about the work disincentive effects of income transfer payments, however, it is hoped that evidence about the magnitude of such effects can make a contribution to the formulation of intelligent income transfer program policy.

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TRADE-OFFS BETWEEN ALTERNATIVE INCOME MAINTENANCE PROGRAMS

By Samuel A. Rea, Jr.*

The problems associated with current welfare programs have brought forth numerous suggestions for reform. The two most basic types of alternative programs are the negative income tax and the wage subsidy. Recently earnings subsidies and programs which combine a wage subsidy with a negative income tax have been added to the list of proposals.[1] Each of these programs attempts to improve the living standard of the poorest members of our society, but that goal is not easily achieved without interfering with other goals such as improving work incentives. In this study a number of alternative programs are compared in light of some important policy objectives. The objectives considered are: (1) transfer income to the poorest individuals in society; (2) minimize the reduction in work effort; (3) minimize the budget cost; (4) offer incentives for education and training; (5) provide horizontal equity; and (6) minimize the real cost. Using estimates of changes in work effort induced by changes in wages and income, the incentive effects of the programs are estimated along with budget costs, real costs, and impacts on different income groups. At the outset, the theoretical advantages and disadvantages of each program are discussed.

I. THE THEORETICAL BACKGROUND

A. Guaranteed Income (GI)

The primary goal of any income maintenance plan is to make the poor better off. In meeting this goal, the first problem is to define who the poor are. Usually the poor are defined to be all those with income below a given level. This poverty line will of course differ for different individuals as their needs are perceived to differ, usually because of family size, and it will change over time as prices increase and living standards change. However the poverty line is defined, one can estimate the amount of income which income support plans would transfer to those below it.

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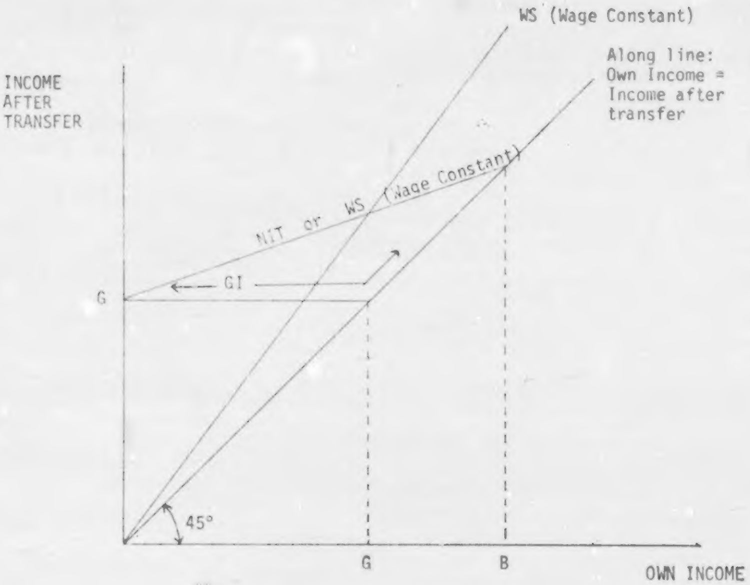
[1] Michael C. Barth and David H. Greenberg, "Incentive Effects of Some Pure and Mixed Transfer Systems," *Journal of Human Resources*, vol. VI, 2 (spring 1971), pp. 149–170. J. R. Kesselman, "A Comprehensive Approach to Income Maintenance: SWIFT," *Journal of Public Economics*, vol. 2, 1 (February 1973), pp. 59–88. Robert H. Haveman, "Work-Conditioned Subsidies as an Income Maintenance Strategy: Issues of Program Structure and Integration," U.S. Congress, Joint Economic Committee, *Studies in Public Welfare*, Paper No. 9 (pt. 1), (Washington, D.C.: U.S. Government Printing Office, 1973), pp. 33–67. U.S. Senate, Committee on Finance, *Social Security Amendments of 1972*, 92d Cong., 2d sess. (Washington, D.C.: U.S. Government Printing Office, 1972), pp. 409–431.

One could imagine a program which would be completely effective in eliminating poverty. All those in poverty would receive benefits equal to the difference between their income and the poverty line. One might label this the guaranteed income (GI). The effect of such a program on income is illustrated in figure 1. No one in the society would receive net income *below* the poverty line, *G* (and no benefit would go to anyone with income *above* the poverty line). This type of program would be ideal in many respects if individuals did not react to it. Unfortunately, the GI abolishes financial incentive to work for its beneficiaries—those whose initial income is below the poverty line. No matter how much they work, their income is constant until their own income exceeds the poverty line income. Those with incomes below the poverty line will stop working altogether and collect the guaranteed income. Some individuals with incomes above the poverty line will also drop out of the labor force. Individuals who respond in this manner will sacrifice some income but will greatly increase their leisure. Thus, although the guaranteed income succeeds in increasing the income of the poorest, it fails to achieve the second goal, that of encouraging the poor to work.

B. The Negative Income Tax (NIT)

The adverse effects on work effort (labor supply) of a guaranteed income flow from the provision that each dollar of earnings reduces benefits by $1. The relationship between the loss of benefits and the additional income is called the benefit-loss rate or the tax rate. For the GI earnings are "taxed" at a 100-percent rate. It has been suggested that income be taxed at a lower rate, 67 percent for instance. This type of program is called a negative income tax. In figure 1 the relationship between own income and total income after the transfer is indicated. Notice that by lowering the tax rate below 100 percent those with incomes above *G* also receive benefits. Everyone with income less than *B* will qualify for the program. *B* is called the break-even income level. Algebraically, *B* equals G/r, where r is the tax rate. For instance, if *G* equals $3,000 and the tax rate is two-thirds, the break-even income level equals $3,000 divided by two-thirds, or $4,500. Since benefits are received by those above the poverty line, *G*, the negative income tax is not efficiently fulfilling the first objective, to transfer the income to the poor. It is also inferior to the guaranteed income with respect to a third objective, that of lowering the budget cost of the transfer program.

FIGURE 1


WS (Wage Constant)
Along line: Own Income = Income after transfer
INCOME AFTER TRANSFER
NIT or WS (Wage Constant)
GI
G
45°
G
B
OWN INCOME

GI - Guaranteed Income

NIT - Negative Income Tax

WS - Wage Subsidy

The negative income tax *reduces* the disincentives associated with the guaranteed income, but it does not *eliminate* them. Those with income levels below B have an incentive to reduce their hours of work and perhaps to drop out of the labor force. Assume that recipients initially have only income from earnings. The negative income tax (NIT) consists of two components, the guarantee, G, and the tax rate, r. Since the guarantee is independent of the individual's hours of work, it has the same effect on labor supply as an equal amount of nonwage income such as dividends, rent, or interest.

Instead of earning W per hour the recipient now earns $(1-r)W$ per hour. For instance, if the tax rate is two-thirds, the recipient earns $(1-0.67)W$ per hour; that is, one-third W per hour. The tax has, in effect, reduced his wage rate. An added hour of work at a \$3 per hour job yields much less than \$3 after subtracting the reduction in the worker's NIT payment. If the tax rate is 0.67, then the net return to the worker is \$1 (or \$3 less the \$2 reduction in the NIT benefit). The NIT recipient can be expected to make his labor supply decision just as if he had nonwage income equal to G and a wage rate equal to $(1-r)W$. Therefore, in order to predict the change in work effort associated with the NIT, one must know how individuals respond to changes in nonwage income and changes in their wage rate.

When an individual has an increase in nonwage income, one expects him to increase his consumption of a wide variety of goods. It seems reasonable (and has empirically been found to be true) that he will also choose to "purchase" more leisure, that is, reduce his hours of work. If the increase in nonwage income is sufficiently large he may drop out of the labor force altogether. The reduction in work effort in response to an increase in nonwage income is called an *income effect*.

When an individual receives an increased *wage rate*, the response is more complicated. The increased wage rate means that with a given number of hours of work he can receive more income. Just as for an increase in nonwage income, this income effect will cause a reduction in hours of work. However, the income effect of an increased wage rate will not induce the recipient to drop out of the labor force entirely because then he would have to give up the advantage of the higher wage rate.

An increased wage makes income "cheaper" in terms of hours of work. At the same time, an increased wage makes an hour of leisure more "expensive" in terms of the forgone income that it costs. This change in the price of leisure (ignoring the increase in income) will induce the individual to work more and have less leisure. Since the individual substitutes income for leisure because of a price change, this is called the ***substitution effect***.

The two effects of a wage change, the income effect and the substitution effect, work in opposite directions—the income effect induces less work, the substitution effect more work. However, we have a great deal of evidence that the income effect is the more forceful for most individuals. Historically hours of work have declined as the real wage rate has increased. Most cross-section studies have also found that those with higher wage rates tend to work fewer hours.[2] The relationship is described as a backward-bending supply curve for labor. The lowest paid workers are the only group likely to increase work in response to a wage increase. The problems of estimating the relationship between hours of labor and wage rates are discussed by Garfinkel elsewhere in this volume.

The negative income tax combines an increase in nonwage income, *G*, with a decrease in the net wage rate (reflecting the benefit reduction caused by earnings). Because of the income effect, the guarantee will cause a reduction in hours of work and labor force participation. The decrease in the net wage rate will cause an increase in hours of work if the supply curve is backward bending, but at very low wage rates the supply curve may not be backward bending. The lower net wage will cause a decrease in the number of people who participate in the labor force. The combined effects of the NIT for an individual with income less than the break-even income level must be to reduce his hours of work and to decrease the probability that he will participate in the labor force.[3] The disincentives to work will be intensified as the tax rate is increased, but these disincentives may affect fewer people since a high tax rate reduces the break-even income level.

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[2] For a review of this literature see Samuel A. Rea, Jr., "The Supply of Labor and the Incentive Effects of Income Maintenance Programs," unpublished Ph. D. dissertation, Harvard University, 1971.

[3] Christopher Green, "Negative Taxes and Monetary Incentives to Work: The Static Theory," *Journal of Human Resources*, III (summer 1968), pp. 280–288.

There are also work disincentives for those with incomes greater than B (see fig. 3). Some of these individuals may be tempted to reduce their hours of work or drop out of the labor force in order to qualify for NIT benefits. They give up some income in order to increase their leisure.

These basic conclusions are not altered if the recipient receives *nonwage* income. In a simple NIT plan this income would be taxed at the same rate as earnings. The individual with nonwage income Y_{nw} would have a net nonwage income increase of G minus rY_{nw} under the NIT and his earnings would be taxed at a rate r.

In order to improve incentives, the H.R. 1 version of the family assistance plan included an earnings exemption. It was proposed that the first $840 of earnings be exempt from benefit reductions (irregular earned income of $30 per quarter plus the first $720 annually of other earnings).[4] As compared to a plan without an exemption, this provision may increase the hours of work of those earning less than $840, and it would increase labor force participation. However, for those earning more than $840, the exemption would reduce hours worked. This is because the exemption is like an increase in nonwage income equal to r times $840 for those earning more than $840. The exemption also increases the cost of the plan and increases the amount of benefits going to higher income individuals.

If the tax rate is lowered, the disincentives are reduced, but the number of individuals qualifying is increased because the break-even income increases. A lower guarantee reduces the break-even income and improves work incentives but lowers the amount going to those without alternative sources of income. This underlying conflict among the objectives of providing adequate incomes for the poor, improving work incentives, and reducing the budget cost cannot be escaped. The next section of this study discusses the way in which alternative plans fulfill these objectives.

C. The Wage Subsidy

A negative income tax produces smaller disincentives than a guaranteed income because the tax on earnings is smaller. In order to provide even greater incentives to work the marginal tax on earnings can be made negative. In other words, an increase in earnings might raise benefits rather than reduce them as with a positive marginal tax on earnings. The wage subsidy is one proposal that embodies this approach. In addition to subsidizing work effort, one might further encourage work by eliminating the guarantee. A wage subsidy provides an increase in benefits as hours of work increase (see fig. 1). To prevent everyone who works from receiving a wage subsidy one would limit participation to those below some wage rate, B_W. In order to offer incentives for the individual with a wage that is less than B_W to increase his wage (through training or job search for instance) the per hour wage subsidy could vary with the wage rate. For instance, the subsidy might be a fraction r_W (sometimes called the subsidy rate) of the difference between the individual's wage and B_W. Algebraically, the wage after the subsidy (W_s) would equal $G_W + (1-r_W)W$, where G_W is the guaranteed wage and W is the unsubsidized wage. The amount of the subsidy per hour is $G_W - r_W W$. Given G_W and the subsidy rate, r_W, the wage at which the wage subsidy equals zero is G_W/r_W. As an example, if the per hour subsidy is equal to 0.75 of the difference between the individual's wage and \$2, then $B_W=\$2$, $r_W=0.75$, and $G_W=\$1.50$. An individual earning \$1 per hour would receive a subsidy of 0.75 of (\$2 − \$1). The individual earning \$1.90 would receive a subsidy of \$0.075 per hour or 0.75 of (\$2—\$1.90). Note that there is a tax of 75 percent on an increase in the wage rate. A worker who increases his unsubsidized wage from \$1.90 to \$2 would find his total net wage rises from \$1.975 to \$2. Given a positive number of hours worked, the relationship between own income and income after the transfer with the wage rate varying is the same as for a negative income tax with $G=G_W\times$(Hours Worked) and $r=r_W$ (see fig. 1).

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[4] The exact provisions of the plan are explained in the supplementary materials.

The wage subsidy and a negative income tax both tax an increased wage. This conflicts with a fourth goal, that the poor be encouraged to undergo training and that employers be allowed a chance to attract employees by offering them a higher net wage.[5] This goal is important because it is an attempt to reduce the number of poor (and transfer recipients) in the long run. Unfortunately, with a lower subsidy rate for a wage subsidy more individuals qualify. Again one cannot escape the basic algebra. The break-even wage (B_W) increases as the subsidy rate (r_W) falls. If the guaranteed wage (G_W) is increased, B_W also increases. There is a basic conflict among the objectives of providing a reasonable guaranteed wage, keeping the tax rate low, and transferring income to those with the greatest need. The problem is analogous to the difficulty of choosing G, r, and B for an NIT.

Hours worked of course will not remain constant after a transfer program is introduced. A wage subsidy as described above, with no tax on nonwage income, would have the same effect on the individual as an increase in his wage rate. As described above, it is possible that an increase in the wage rate will reduce the number of hours worked. In other words, those receiving the wage subsidy will use some of the additional income to purchase leisure. Because the return from work has increased, the reduction in the individual's hours worked because of a wage subsidy will always be less than the reduction caused by a negative income tax if the same amount is transferred to him.[6] A wage subsidy will increase the amount of labor force participation because those who are out of the labor force will be tempted to enter by the higher net wage rate. Those in the labor force will remain because the subsidy is conditioned on labor force participation (although they might withdraw from the labor force in later years). The wage subsidy is clearly superior to a negative income tax with regard to hours of work and labor force participation.

The work incentive effects of the NIT and the WS can be improved by increasing the rate of taxation on nonwage income. As the tax on nonwage income is increased, hours of work and labor force participation increase because of the income effect. A higher tax on nonwage income also lowers the break-even income under an NIT for those with income from sources other than earnings,[7] and therefore

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[5] Samuel A. Rea, Jr., "Investment in Human Capital and Income Maintenance Programs," unpublished manuscript, 1973.

[6] Jonathan Kesselman, "Incentive Effects of Transfer Systems Once Again," *Journal of Human Resources*, vol. VIII, 1 (winter 1973), pp. 119–129.

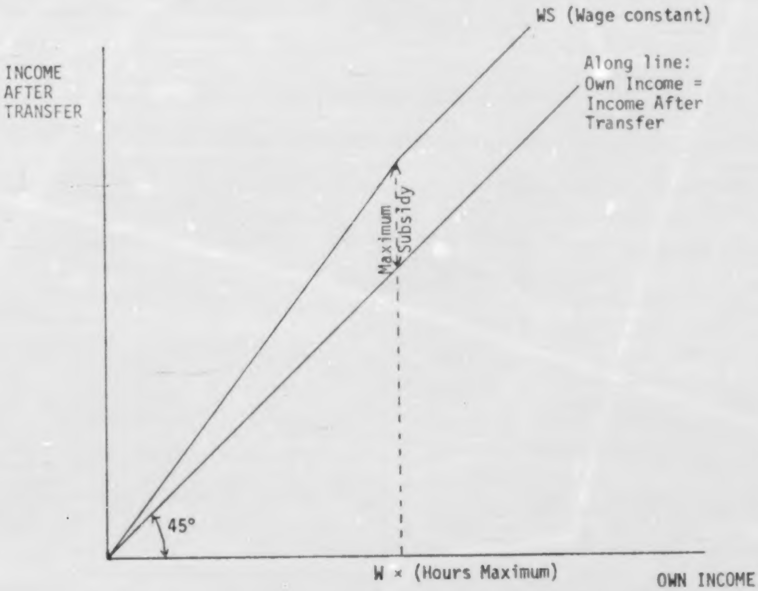
[7] If r_{NW} is the tax rate on nonwage income, Y_{NW}, and r_E is the tax rate on earnings

$$B=(G/r_E)-\left(Y_{NW}\,\frac{[r_{NW}-r_E]}{r_E}\right)$$

reduces the amount that is paid to those with incomes above *G*. The disadvantages of a very high tax rate on nonwage income are that it discourages saving by the recipient and payments from private sources, such as child support and alimony.

Although a wage subsidy is likely to be superior to a negative income tax in regard to incentives, figure 1 illustrates that for an individual with a given wage the subsidy is positively related to income. This conflicts with the goal of helping most the poorest members of society. On the high income side it may be desirable to eliminate the per hour subsidy after a given number of hours have been worked. With this restriction the total amount paid remains constant if a recipient increases his work beyond a given maximum hours. The effect of this on an individual's income is shown in figure 2. The drawback to this feature is that for those working more than the maximum hours, the subsidy is equivalent to a lump-sum payment which induces them to reduce their hours of work. Those working the maximum hours would increase their hours of work if the maximum were eliminated. There is no difference in the effect on labor force participation.

FIGURE 2.—Wage subsidy.



WS - Wage Subsidy

D. Combining the Negative Income Tax and the Wage Subsidy

Those who are unable to work or who cannot find work receive no benefits under a wage subsidy. This is a serious defect in any work-conditioned subsidy plan. For this reason wage subsidies have been proposed in combination with a negative income tax and with public employment plans. Those who are unemployed would be guaranteed a job at a fixed wage rate. Those who cannot work (or can only work part time due to family obligations) would receive some guaranteed income. This type of combination plan reintroduces the administrative problem of categorizing individuals. Kesselman suggests that the categorization be based on reasonably objective criteria such as physical or psychological ability to work and presence of pre-school children.[8] A public employment program is a crucial part of such a plan because the administrative difficulty of differentiating between those who cannot find a job and those who do not want a job is immense.

Because it is difficult to categorize individuals administratively, a combination of a negative income tax for those who are unlikely to work under any circumstances with a wage subsidy could be a superior alternative to either individual plan. Unfortunately, such a plan would be expensive. Zeckhauser and Schuck [9] propose a plan under which the individual chooses a program, the NIT or the WS, in which to participate (fig. 3). This eliminates administrative discretion, but it is less effective in terms of budget cost and work incentives because some workers who could receive a wage subsidy (those along segment *AC*) might reduce their hours of work in order to qualify for the negative income tax (segment *GA*). The Zeckhauser-Schuck wage subsidy makes up half of the difference between the market wage and \$3. An additional feature of the plan is that no one with income greater than \$5,500 could receive a subsidy. The intention of this feature is to reduce the budget cost of the program and to avoid subsidizing those with higher incomes. The drawback is that there is a strong incentive for those earning more than \$5,500 to reduce their hours worked in order to qualify for this subsidy. Again the fundamental dilemma of any transfer program appears. Attempts to limit the amount paid to higher income groups almost always produce work disincentives. This is true whether it be a higher tax rate under a negative income tax, an hours maximum under a wage subsidy, or maximum income under a wage subsidy.

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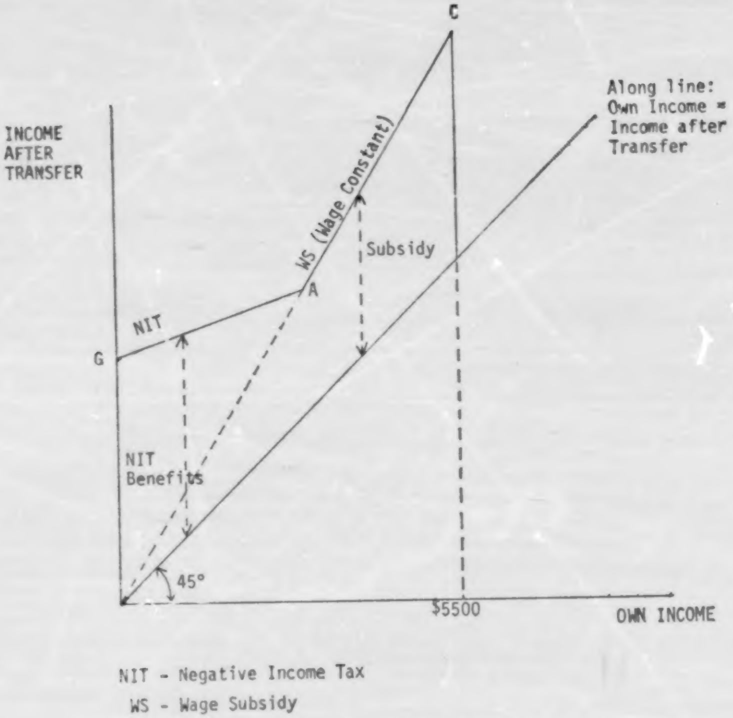
[8] Kesselman, "A Comprehensive Approach to Income Maintenance: SWIFT," op. cit.

[9] Richard Zeckhauser and Peter Schuck, "An Alternative to the Nixon Income Maintenance Plan," *The Public Interest*, No. 19 (spring 1970), pp. 120–130.

FIGURE 3.—Zeckhauser and Schuck plan.


C
Along line:
Own Income = Income after Transfer
INCOME AFTER TRANSFER
WS (Wage Constant)
Subsidy
A
NIT
G
NIT Benefits
45°
$5500
OWN INCOME
NIT - Negative Income Tax
WS - Wage Subsidy

There have also been suggestions to mix a negative income tax with a wage subsidy.[10] Rather than using different programs for different individuals, one could design a single plan in which everyone would be eligible for an NIT guarantee (G), subject to a tax rate (r) on all income, and, in addition, be eligible for a wage subsidy. Wage subsidy payments would be included in income taxable for NIT purposes. Net income under such a mixed program would equal

$$Y = G + (1-r)[G_W + (1-r_W)W]L + (1-r)Y_{NW}$$

where Y_{NW} is nonwage income and L is hours worked. Notice that in effect the tax rate on earnings is a function of the wage rate. That is, the net gain in income from an added dollar of earnings depends on the wage rate. At very low wage rates the individual's wage is subsidized, and additional work increases the total subsidy.

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[10] Barth and Greenberg, op. cit. In Kesselman's proposal the mixture of the two programs occurs for families eligible for the NIT in which an individual qualifies for a wage subsidy.

For those whose wage exceeds

$$\frac{(1-r)G_W}{1-(1-r)(1-r_W)}$$

additional earnings are taxed just as under a negative income tax. For reasonable parameters this critical wage is so low that few individuals would receive a subsidized wage.[11] For those above this wage the plan is in effect a negative income tax with a tax rate on earnings that increases with the wage rate.[12]

Those with higher wage rates have a lower break-even income and fewer hours worked at the break-even income. This mixed plan is less generous to those with the potential for higher incomes because of their higher wage rates. If those with a higher wage rate become unemployed for part of a year, they receive smaller benefits than those with the same income but a lower wage rate. Of course, the higher wage individual has more leisure if unemployment is viewed as such.[13] Since the guarantee is the same regardless of the wage, the difference in benefits occurs only for those who work. The incentive effects are identical to those of a negative income tax for a fixed wage rate above the critical level. The desirability of such a program as opposed to a simple NIT rests largely on whether it is felt that those with higher earning potential (higher wage rate) should be taxed at a higher rate.[14] This introduces a fifth objective, horizontal equity. The program (NIT plus wage subsidy) is not equitable in terms of equal benefits for equal income, but it may be equitable if one includes the extra leisure or the extra potential earnings of the higher wage individual. A simple negative income tax gives equal benefits for equal income, but individuals with higher wages or nonwage income have more leisure and are therefore better off.

E. The Earnings Subsidy (ES)

The wage subsidy, even if it is limited to those who are "able" to work, has two drawbacks that might be remedied. First, it pays substantial benefits to those who work long hours but are not poor. Second, it is likely to tax a wage increase heavily. This reduces the individual's incentive to look for higher paying jobs and to invest in education or training. For this reason an earnings subsidy has been proposed. [15] The earnings subsidy (ES) would operate exactly like a wage subsidy for an individual with a given wage rate. For instance, the earnings subsidy might be 25 percent. This would be equivalent to a 25-percent increase in the wage rate. The difference occurs when one compares individuals with different wage rates. With a WS the subsidized wage does not increase as fast as the wage rate. With an ES the subsidized wage increases more than the wage rate in absolute terms. This may increase the incentive to improve one's wage rate.[16] On the other hand it means that those with higher incomes receive greater benefits. This conflicts with the objectives of reducing the budget cost and transferring income to the poorest individuals. To reduce the amount transferred to higher income individuals a tax rate could be imposed at some income level, *A*. For instance, if there is a 25-percent ES, one might tax earnings over $3,000 at a 50-percent rate. An individual with $3,000 in earnings would receive $750 in subsidies, and the break-even income would be $4,500. The plan is represented diagramatically in figure 4.

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[11] If $r=.5$, $r_W=.5$, and $G_W=\$1.50$, the critical wage is $0.50 per hour.

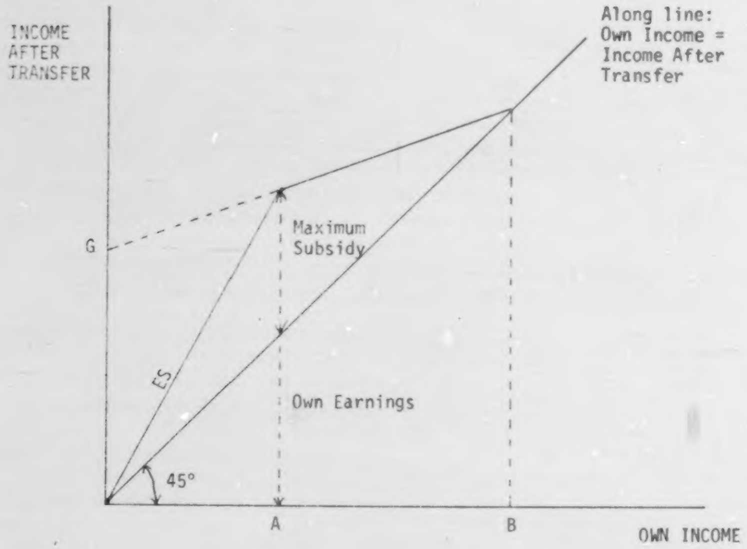
[12] Aaron's plan also has this feature. Henry J. Aaron, *Why is Welfare So Hard to Reform?* (Washington, D.C.: The Brookings Institution, 1973).

[13] For married men unemployment has little leisure value while for married women about half of measured unemployment is in fact leisure. Samuel A. Rea, Jr., "Unemployment and the Supply of Labor," *Journal of Human Resources*, forthcoming.

[14] The real cost is likely to be higher than for an NIT. See below.

[15] Haveman, op. cit., Finance Committee op. cit.

FIGURE 4.—Earnings subsidy.



ES - Earnings Subsidy

For any particular individual the earnings subsidy increases his labor force participation just as a WS does. His hours will decrease just as for a WS if his income is less than *A*. If he has income between

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[16] The increased incentives occur unambiguously only if the per hour subsidy is the same for the WS and the ES. See Rea, "Investment in Human Capital and Income Maintenance programs," op. cit.

A and *B*, he will reduce his hours more than under a WS. In fact it can be seen in figure 4 that in this range the earnings subsidy is just like an NIT with a guarantee, *G*. It reduces hours worked and incentives for wage improvement just like an NIT but makes it financially more worthwhile to remain in the labor force (or to enter it) than does the NIT. The ES may or may not be superior to a WS (fig. 2) in terms of costs, but it has inferior work incentives. It is superior to an NIT in terms of hours worked only for individuals earning less than *A*. It is inferior to an NIT in terms of help to the poor; those with low earnings receive little additional income.

While it is advantageous to offer incentives for increased job training, the effect of the ES is to pay larger subsidies to those with higher wage rates for a given number of hours worked. For a given level of earnings the individual with the higher wage rate works less but receives the same total subsidy as a person with a lower wage. This characteristic is also true of the NIT. If one's concept of horizontal equity measures earnings capacity (or includes leisure), the ES has less horizontal equity than the WS. One could combine a wage subsidy with a tax on income over some level. This type of program would have the same work incentive effects as the ES given the wage rate but would transfer more income to low wage individuals and would reduce the incentives to improve one's wage rate. This illustrates the conflict between the goal of providing incentives to undergo training and the goal of equal treatment of those with equal income-earning potential. The same problem occurs if one compares a simple negative income tax with a plan that increases the tax rate as the wage increases. The mixed plan reduces the incentives to increase the wage rate but partly takes earning potential into account, while the simple NIT ignores differences in earning capacities (or leisure) for individuals with equal incomes and improves incentives to increase the wage.

F. The Real Cost of Transfers

Economists add a sixth goal for transfer programs. The program should minimize what is called the welfare cost or real cost. When income is transferred from one individual to another in a lump-sum form (the amount of the transfer does not depend on the recipient's income), the recipient will reduce his work. Although his contribution to the production of goods and services in the economy is reduced, his leisure increases.[17] Since the increased leisure is worth at least as much to the individual as the goods no longer produced, one can say that there is no real cost to the transfer. However, whenever income is transferred only to those with low incomes, there is an implicit tax on additional earnings as shown for the GI and the NIT. The result is that the individual substitutes leisure for work. This substitution, which is caused by an alteration in the net wage rate of the recipients, imposes a real cost on society. For a given amount transferred, the individual will always be better off with a lump-sum transfer. The real cost of a transfer that alters the net wage rate is the difference between the amount transferred and the amount of a lump-sum transfer that would make the recipient just as well off. The real cost depends on the absolute value of the change in the net wage rate and the size of the substitution effect.[18] In the case of a wage subsidy there is a real cost because the individual is induced to substitute work for leisure because of the higher net wage rate. As with the case of an income-conditioned program he could be made better off with a lump-sum transfer of equal amount that did not depend on his hours of work. The real cost is greatest for those programs with the largest tax rates or the largest wage subsidy, or programs which cover more individuals. There is also a real cost associated with the taxes that are used to finance the income maintenance programs.

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[17] The opposite response occurs for the individual being taxed to provide the transfer.

The various programs discussed above are compared in the next section in light of four of the objectives mentioned above: (1) transfer income to the poorest individuals; (2) minimize the reduction in work effort; (3) minimize the budget costs; and (4) minimize the real cost. The goals of horizontal equity and incentives for wage increases cannot be measured by these simulations. Additional important objectives such as family stability and minimum administrative cost are not discussed.

II. ESTIMATION AND SIMULATION

In order to predict the response of individuals to the various plans discussed in the previous section, it is first necessary to estimate their response to changes in wage rates and unearned income. This is very difficult because of the nature of the data and the nature of the experiences that one might observe. Garfinkel has highlighted many of the difficulties.

The hours responses used in this study were estimated using the Current Population Survey. This survey, a sample of about 100,000 persons age 14 and over, is the basis for labor force data such as the unemployment rate. The particular survey used (March 1967) covers work experience and income during 1966. The response of hours worked was estimated for those age 25 and over. The estimation techniques are discussed elsewhere.[19]

The central assumption required for cross-section estimates like these is that the difference in labor supply between two individuals who are otherwise similarly situated is due to differences in wage rates and nonwage income. In other words one must assume that a change in an individual's wage rate or nonwage income will cause changes in work behavior similar to the observed differences in work behavior between individuals who have different wage rates and nonwage income. Another assumption is that people respond to nonwage and earnings-related income provided through transfer programs as they would to income from private sources.

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[18] The real cost $= \frac{1}{2}[(dw)^2 \times (\text{substitution effect})]$. See Samuel A. Rea, Jr., "Incentive Effects of Alternative Negative Income Tax Plans," Institute for the Quantitative Analysis of Social and Economic Policy, University of Toronto, Working Paper No. 7209, 1972.

[19] Rea, "The Supply of Labor and the Incentive Effects of Income Maintenance Programs," op. cit.

The estimated supply responses are generally consistent with the theory outlined in the previous section. An increase in nonwage income reduces hours worked, implying a negative income effect. The substitution effect is positive in most cases as required by economic theory. An increase in the wage rate reduces hours worked, indicating the income effect is larger than the substitution effect. In families in which both husband and wife work, an increase in the wage rate of one partner reduces the hours worked of both.

With the estimated responses it is possible to predict the effects of any program on the hours worked of each person in the sample and to project the effects to the entire population over age 25. Once the labor supply has been predicted, the amount of benefits for each individual or family in the sample can be calculated.[20] The aggregate response and cost of each plan are obtained by weighting each family or person by the inverse of the sampling ratio, which averages about 1 in 1,300.

In simulating the various plans a rather crude effort was made to remove AFDC payments from the recipients' income before calculating supply responses and program costs. This has the effect of slightly reducing the disincentives of the programs shown because of the income effect, but it increases the cost of the new program. The costs that are shown are gross budget costs of the proposed programs assuming AFDC is eliminated. The net cost of the programs equals the gross cost in the tables minus the saving from the elimination of AFDC. This saving will be rather small in relation to the total cost shown because many AFDC recipients in 1966 were not in the labor force.

The aggregate costs produced by the simulations must be interpreted with caution. First, they are based only on those who are 25 and over and are in the labor force. Those below 25 and those out of the labor force are not included unless they are children (under 18) of those who are included. Second, the costs are in 1966 terms. In order to predict the costs in a later year one must take account of changes in prices, real incomes, and the income distribution. Third, changes in market wage rates that could follow the introduction of these programs are not considered. Fourth, changes in labor force participation could also take place, but this effect is not estimated. In spite of these caveats the results are useful because they allow one to compare programs using the criteria discussed in the previous section.

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[20] The technique for predicting the supply of those above the break-even income level is explained in Rea, "Incentive Effects of Alternative Negative Income Tax Plans," op. cit.

Table 1.—*Negative income tax, age 25 and over and in the labor force, 1966*

Plan number	Tax rate	Annual guarantee		Group [1]	Thousands of filing units	Gross budget cost (millions)		Percent decline in work hours [2]	Recipients below poverty line (percent)		Percent of benefits to those initially [3]			Real cost (millions)
		Adult	Child			If program caused no work reduction	Including cost of work reduction		Before program	After program	Below guarantee	Guarantee to break-even	Above break-even	
1	0.5	$500	$300	M & F, NMSP	2,245	$1,092	$1,213	14	95	98	73	26	1	$79
				M, MSP, SNW	1,434	814	972	11	98	89	42	56	2	94
				M & F, MSP, SW.	1,109	667	697	2	92	87	51	48	1	37
								6						
				Total	4,788	2,573	2,882	9	95	93	57	42	1	210
2	0.33	750	450	M & F, NMSP	5,416	3,471	3,785	12	57	46	57	42	1	170
				M, MSP, SNW	7,765	6,826	7,172	6	17	3	23	76	1	213
				M & F, MSP, SW.	5,474	4,712	4,842	3	20	4	28	71	1	362
								6						
				Total	18,655	15,009	15,799	6	29	16	33	66	1	745
3	0.5	750	450	M & F, NMSP	3,636	2,385	2,653	14	84	81	74	25	1	198
				M, MSP, SNW	3,616	2,916	3,392	11	36	12	44	54	2	317
				M & F, MSP, SW.	2,455	2,150	2,551	10	44	17	48	51	1	207
								14						
				Total	9,707	7,451	8,596	12	56	39	55	44	1	722
4	0.67	750	450	M & F, NMSP	2,633	1,817	2,002	14	92	98	89	9	2	207
				M, MSP, SNW	2,020	1,544	2,058	16	64	48	63	33	4	289
				M & F, MSP, SW.	1,441	1,239	1,856	19	74	55	60	34	6	158
								24						
				Total	6,094	4,600	5,916	17	78	71	71	25	4	654

TABLE 1.—*Negative income tax, age 25 and over and in the labor force, 1966*—Continued

Plan number	Tax rate	Annual guarantee		Group [1]	Thousands of filing units	Gross budget cost (millions)		Percent decline in work hours [2]	Recipients below poverty line (percent)		Percent of benefits to those initially [3]			Real cost (millions)
		Adult	Child			If program caused no work reduction	Including cost of work reduction		Before program	After program	Below guarantee	Guarantee to breakeven	Above breakeven	
5	1.00	750	450	M & F, NMSP	1,974	1,262	2,849	100	87	100	81	0	19	1,168
				M, MSP, SNW	982	683	2,560	100	90	99	75	0	25	672
				M & F, MSP, SW.	854	578	2,040	100	80	98	73	0	27	1,313
								100						
				Total	3,810	2,523	7,449	100	86	99	77	0	23	3,150
6	0.5	1,000	600	M & F, NMSP	4,924	4,130	4,540	13	63	40	77	22	1	342
				M, MSP, SNW	6,322	7,091	7,830	9	21	1	45	54	1	486
				M & F, MSP, SW.	4,350	4,964	6,021	15	25	1	49	49	2	572
								18						
				Total	15,596	16,185	18,391	13	35	13	54	45	1	1,400
7	0.67	1,000	600	M & F, NMSP	3,696	3,168	3,490	13	83	69	90	8	2	380
				M, MSP, SNW	3,728	3,846	4,991	16	35	2	61	36	3	613
				M & F, MSP, SW.	2,678	2,842	4,868	30	40	2	56	35	9	523
								35						
				Total	10,102	9,856	13,349	21	54	26	67	28	5	1,516
8	0.67	1,250	750	M & F, NMSP	4,688	4,858	5,296	14	67	31	92	7	1	595
				M, MSP, SNW	5,689	7,705	9,362	14	23	0	67	31	2	864
				M & F, MSP, SW.	4,194	5,082	9,196	32	26	0	57	35	8	1,071
								37						
				Total	14,571	17,645	23,854	22	38	10	69	27	4	2,530

9	0, 0.67	H.R. 1	H.R. 1	M & F, NMSP	2,012	2,237	2,823	10	61	25	76	23	1	488
				M. MSP, SNW	2,077	1,757	2,471	16	46	20	44	51	5	407
				M & F, MSP, SW.	1,435	1,377	2,449	29	53	33	39	51	10	246
								36						
				Total	5,524	5,371	7,743	20	53	25	54	41	5	1,141
10[4]	0, 0.67	H.R. 1	H.R. 1	M & F, NMSP	4,754	3,372	4;232	14	64	36	63	35	2	742
				M, MSP, SNW	2,971	2,245	3,100	16	42	17	43	52	5	592
				M & F, MSP, SW.	2,129	1,874	2,800	18	50	25	42	51	9	284
								25						
				Total	9,854	7,491	10,132	18	54	28	51	44	5	1,618
11[5]	0, 0.67	H.R. 1 Plus 33 percent	H.R. 1 Plus 33 percent	M & F, NMSP	5,867	5,930	6,537	12	53	14	73	26	1	879
				M, MSP, SNW	4,940	5,039	6,682	16	26	1	46	51	3	1,055
				M & F, MSP, SW.	3,673	3,819	6,939	32	30	1	41	48	11	848
								35						
				Total	14,480	14,788	20,158	22	38	6	53	42	5	2,782
12[6]	0, 0.67 Subsidy	H.R. 1	H.R. 1	M & F, NMSP	2,517	3,777	3,686	6	49	21	61	27	12	286
				M, MSP, SNW	2,970	4,014	4,230	15	32	13	28	44	28	334
				M & F, MSP, SW.	2,562	5,565	5,828	32	27	7	20	38	42	579
								20						
				Total	8,049	13,356	13,744	19	36	14	33	37	30	1,199

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[1] M=male, F=female, MSP=married-spouse-present, NMSP not married-spouse-present, SW=wife in labor force, SNW=wife not in labor force.

[2] Where applicable: Husband's response; wife's response.

[3] The guarantee and break-even levels are identical for plans 9, 10 and 12.

[4] H.R. 1 extended to families without children.

[5] H.R. 1 with guarantee raised by 33 percent, and extended to families without children.

[6] Zeckhauser-Schuck plan.

III. Trade-Offs

The President's Income Maintenance Commission recommended a negative income tax plan that would guarantee $2,400 for a family of four and tax income at a 50-percent rate. The plan would offer $750 for the first two adults and $450 for each child in a family. Suppose that this plan was instituted in 1966 as a guaranteed income with a 100-percent tax on each family's income. The income filing unit is assumed to be an individual (without a spouse present) over 25 and his or her children under 18 or a married couple together with their children under 18.[21] In table 1 the effects of such a plan are illustrated. (Plan No. 5.) The budget cost of the GI would be $2.5 billion if recipients worked as before, but since work would not increase their income, it is expected that they would stop work. Their withdrawal from work would triple costs, up to $7.4 billion. Furthermore the number of filing units below the poverty line actually *increases* because of the plan.[22] This occurs because 23 percent of the recipients (940,000 filing units) initially had incomes greater than the guarantee. They choose to give up an average of $2,070 per year in order to greatly increase their leisure and receive the guaranteed income, which averaged $1,930 for this group. The guaranteed income produces extreme work disincentive effects and imposes very high real costs. On the other hand a high percentage of the benefits go to those with low incomes.

If the tax rate is lowered, one has a negative income tax. With a tax rate of 67 percent (plan 4 in table 1) the negative income tax has a larger number of recipients but a lower budget cost because the incentive effects are not so severe. It also has a lower real cost. However, the NIT pays a higher proportion of the benefits to those not in poverty and those earning more than the guarantee.

A comparison of plans 1 through 8 in table 1 reveals the trade-offs between the objectives outlined above. As the guarantee level is raised, the program is more adequate in helping the poorest familes, but many more recipients are added and the budget cost and the real cost increase significantly. In addition more benefits are paid to those who are above the poverty line. As the tax rate is lowered with the guarantee constant, the number of recipients, the budget cost (see fig. 5), and the real cost also increase. The lower the tax rate the smaller is the proportion of the income going to those below the poverty line.

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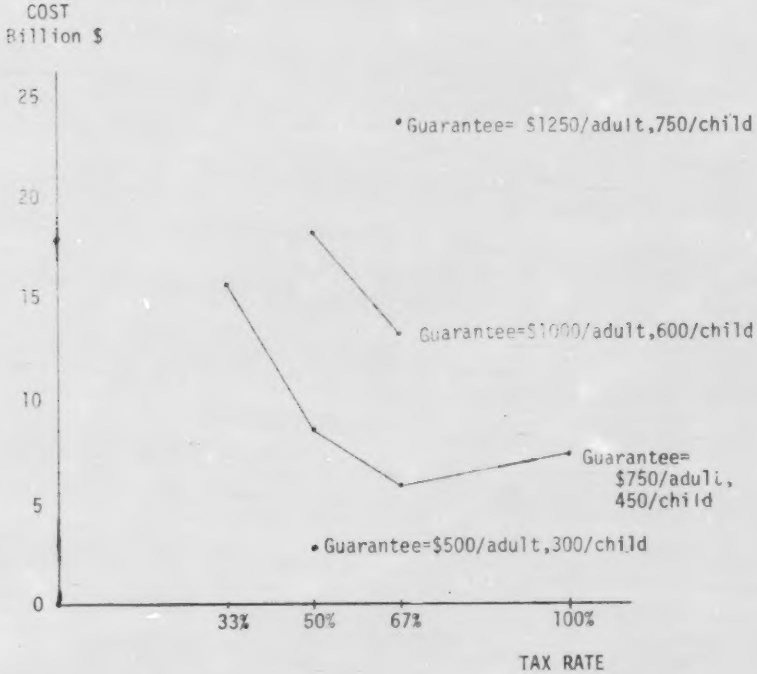
[21] The definition of the filing unit is important, particularly the treatment of college-age youth. See Rea, ibid., William A. Klein, "Familial Relationships and Economic Well-Being: Family Unit Rules for a Negative Income Tax" *Harvard Journal on Legislation*, vol. 8 (1971), pp. 361–405.

[22] The U.S. Bureau of the Census poverty line was adjusted to 1966 prices. U.S. Bureau of the Census, "24 Million Americans—Poverty in the United States: 1969," *Current Population Reports, Series P–60*, No. 76 (Washington, D.C.: U.S. Government Printing Office, 1970), p. 18. The poverty line used in this study differs slightly from the official line because income is defined in this study on an individual or couple basis not on a household basis.

FIGURE 5.—Negative income tax, gross budget cost.



As the guarantee increases or the tax rate decreases the number of recipients and the budget cost increase rapidly because the higher income brackets are more densely populated. For instance, as the tax rate decreases from 67 percent to 33 percent for a $2,400 guarantee (family of four), the cost nearly triples. As the guarantee doubles from $1,600 to $3,200 for a family of four with a 50-percent tax rate, the cost increases sixfold. A 33-percent increase in the H.R. 1 guarantee doubles the cost of the program.

The aggregate incentive effects also respond to changes in the parameters of the programs. As the guarantee increases the decline in hours worked becomes more severe. Not only does this conflict with our objective of maintaining the work effort of the recipients, but it also increases the budget cost of the program. The maximum total reduction in hours worked for the NIT programs simulated is 22 percent.

The percentage reduction in hours is generally much more sensitive to changes in the tax rate than to changes in the guarantee. As the tax rate increases, the hours reduction increases. As long as the tax rate is below 100 percent, decreases in the work disincentives can only be obtained at considerable cost in terms of other objectives. For instance a decline in the tax rate from 67 percent to 50 percent with a $2,400 guarantee (family of four) reduces the hours decline from 17 percent to 12 percent. However, it raises the budget cost from $5.9 billion to $8.6 billion and the number of filing units by 60 percent. In addition the percentage of benefits going to those with incomes initially below the guarantee falls from 71 percent to 55 percent. Even the real cost increases because of the increased number of recipients.

As the tax rate increases, a larger percentage of the recipients with incomes above the break-even income level choose to reduce their earnings in order to receive benefits. For plan 7, 11 percent of the total filing units initially had income above the break-even level ($4,776 for a family of four). They receive 5 percent of the benefits. This percentage is rather low when compared to the share of benefits from a wage or earnings subsidy that go to high income individuals and families. This conclusion seems to run counter to the earlier results showing a higher income effect than wage rate effect. To reconcile these findings, note first that all recipients in this study have private income. Thus, increasing the guarantee in NIT programs with high tax rates adds little total income to most recipients. On the other hand, lowering the tax rate will make a substantial difference for the bulk of recipients who have private income and will increase the share of working recipients.

The H.R. 1 version of the family assistance plan (plan 9) has the same guarantee for a family of four and the same tax rate as plan 4, but it is restricted to families with children. The H.R. 1 plan differs from plan 4 in that the guarantee per child falls as the number of children increases. It also has a $840 earnings exemption (including $30 per quarter in irregular earnings) and a 100-percent tax on nonwage income over $240 per year. Plan 10 is the basic H.R. 1 plan extended to families without children. If one compares it to plan 4, one can see that it is substantially more expensive than the simpler negative income tax. The exemption of $840 is largely responsible for the cost increase since it is equivalent to a $560 increase in the guarantee for all of those earning over $840. The disincentives are also slightly larger under plan 10. In general the reduction in hours for the large number of recipients above the exemption level overwhelms any increase for those earning less than $840. The exemption not only increases costs significantly, it offers no improvement in incentives.

The Zeckhauser-Schuck plan (plan 12) is a combination of H.R. 1 (plan 9) and a wage subsidy. The idea is to provide adequate income for the poor who are unable to work while encouraging the labor supply of those who can work. The improvement in hours worked over H.R. 1 is slight, but the cost is more than double. In addition substantially more benefits go to those with higher incomes.

The wage subsidy per hour was defined to be $Gw—rwW$ where GwW is the guaranteed wage and rw is the subsidy rate. The wage subsidies were simulated with a variety of guaranteed wages and subsidy rates. In addition some other conditions of the wage subsidy programs were varied. Alternative maximum hours restrictions and tax rates on nonwage income were considered. A provision to allow only the head of the family to qualify for the subsidy was included in most of the simulations. The family head was considered to be the husband unless he was out of the labor force. The effect of also allowing the wife to qualify was simulated for purposes of comparison.

The effects of changes in the parameters of a wage subsidy are shown in table 2. As the guaranteed wage is increased, the number of recipients, the budget cost, and the welfare cost increase significantly (compare plans 13 and 15 and plans 20 and 21). For instance, as the guaranteed wage rises from $1 per hour to $1.50 per hour (with a 50-percent subsidy rate) the budget cost triples and the number of filing units doubles. The percentage of the benefits going to those with income less than $2,400 for a family of four declines from 36 percent to 21 percent. Given the tax rate, an increase in G_W helps low wage individuals, but it also allows more high wage individuals to qualify. There is no uniform pattern in the response of disincentives to changes in the guaranteed wage.

TABLE 2.—*Wage subsidy,[1] age 25 and over and in the labor force, 1966*

Plan number	Guaranteed wage	Subsidy rate	Tax on nonwage income R_{NW}	Maximum hours	2 filing in family	Group [2]	Thousands of filing units	Gross budget cost (millions): If program caused no work reduction	Gross budget cost (millions): Including cost of work reduction	Percent decline in work hours (increase) [3]	Percent of recipients: Above hours maximum	Percent of recipients: Below poverty line: Before program	Percent of recipients: Below poverty line: After program	Percent of benefits to those initially [4]: Below guarantee	Percent of benefits to those initially [4]: Between guarantee and break-even	Percent of benefits to those initially [4]: Above break-even	Real cost (millions)
13	$1.50	0.5	1	2,080	No	M & F, NMSP	9,756	$11,283	$11,467	1.0	20	32	7	24	22	54	$1,144
						M, MSP, SNW	6,293	5,775	5,841	3.7	76	19	6	24	40	36	187
						M & F, MSP, SW.	8,552	8,842	8,817	4.4	72	12	3	14	26	60	152
										3.8	1						
						Total	24,601	25,900	26,125	3.1	52	22	5	21	28	51	1,483
14	1.50	.5	1	2,080	Yes	M & F, MSP, SW.	14,982	23,353	22,320	4.1	68	7	1	8	18	74	542
										3.9	1						
						Total	31,031	40,411	39,628	3.3	37	17	4	16	24	60	1,873
15	1.00	.5	1	2,080	No	M & F, NMSP	6,057	4,376	4,417	.7	25	48	20	38	26	36	308
						M, MSP, SNW	2,443	1,567	1,582	2.6	86	42	2[illegible]	46	42	12	34
						M & F, MSP, SW.	3,950	2,671	2,664	2.5	82	23	11	26	34	40	33
										1.8							
						Total	12,450	8,614	8,663	1.7	55	39	18	36	31	33	375
16	1.00	.5	1	2,496	No	M & F, NMSP	6,065	4,515	4,558	(.5)	4	48	20	37	27	36	449
						M, MSP, SNW	2,465	1,826	1,828	1.9	47	42	22	46	42	12	100
						M & F, MSP, SW.	3,988	3,098	3,078	1.7	46	23	10	25	35	40	72
										1.6							
						Total	12,518	9,439	9,464	1.3	26	39	17	35	32	33	621

17	1.00	.5	1	1,768	No	M & F, NMSP	6,029	4,038	4,082	3.1	61	48	21	39	26	35	182
						M, MSP, SNW	2,417	1,332	1,338	2.8	94	42	27	47	41	12	11
						M & F, MSP, SW.	3,895	2,272	2,269	2.4	89	23	13	26	35	39	17
										1.7							
						Total	12,341	7,642	7,689	2.6	76	39	20	36	32	32	210
18	1.50	.75	1	2,080	No	M & F, NMSP	6,373	6,872	6,998	0	24	47	13	36	28	36	738
						M, MSP, SNW	2,603	2,490	2,559	3.3	85	41	17	45	43	12	124
						M & F, MSP, SW.	4,189	4,249	4,254	3.4	80	23	7	25	35	40	105
										2.6							
						Total	13,165	13,611	13,811	1.9	54	38	12	34	33	33	967
19	1.50	.75	1	None	No	M & F, NMSP	6,381	7,120	7,258	(1.5)	0	47	12	36	27	37	1,205
						M, MSP, SNW	2,645	3,068	3,076	0	0	41	13	44	44	12	403
						M & F, MSP, SW.	4,242	5,327	5,257	.7	0	23	5	25	35	40	301
										1.3							
						Total	13,268	15,515	15,591	(.1)	0	38	10	34	33	33	1,909
20	1.50	.75	0	2,080	No	M & F, NMSP	7,393	8,392	8,045	3.3	21	42	12	32	26	42	624
						M, MSP, SNW	3,391	3,545	3,459	4.7	70	34	13	36	45	19	75
						M & F, MSP, SW.	5,221	5,616	5,506	4.2	69	20	5	22	33	45	89
										3.1							
						Total	16,005	17,553	17,010	3.7	47	33	10	30	32	38	788
21	1.00	.75	0	2,080	No	M & F, NMSP	4,461	3,479	3,271	5.8	18	62	28	45	30	25	179
						M, MSP, SNW	1,610	1,268	1,218	6.1	57	52	31	51	40	9	20
						M & F, MSP, SW.	2,539	2,047	1,983	4.4	61	35	18	33	37	30	19
										3.1							
						Total	8,610	6,794	6,472	4.9	38	52	26	42	34	24	218

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[1] The wage after the subsidy equals $G_w+(1-r_w)W$.

[2] M=male, F=female, MSP=married-spouse-present, NMSP=not married-spouse-present, SW=wife in labor force, SNW=wife not in labor force.

[3] Where applicable: Husband's response; wife's response.

[4] The guarantee and break-even levels are for plan 3.

As the subsidy rate (r_W) decreases, the number of recipients, the budget cost and the real cost increase (compare plans 13 and 18). As the subsidy rate decreases from 75 percent to 50 percent with a $1.50 guaranteed wage, the break-even wage increases from $2 to $3 and the budget cost doubles. When the subsidy rate is increased, the work disincentives are less and the percentage of the recipients below the poverty line is greater. On the other hand there are diminished incentives for increasing one's wage rate. This latter objective clearly conflicts with the objectives of minimizing the costs, minimizing the disincentives, and distributing as high a percentage as possible to the poorest people.

The wage subsidy is inferior to the negative income tax with respect to the objective of transferring income to the poorest members of society. As a reference point, the guarantee level and break-even income for the Income Maintenance Commission plan (plan 3) are used. The recipients are classified as to whether they were below G ($750 per adult, $450 per child), between G and B (which equals $2 \times G$), or above B. Plan 3 pays out only 1 percent of the benefits to those above the break-even income ($4,800 for a family of four) while the wage subsidies pay up to 60 percent of the benefits to this group. Even the most modest subsidy (plan 21) gives 24 percent of the benefits to those in the highest income classification. The number below the poverty line is also much lower than for a negative income tax.

As an example of the differences between a negative income tax and a wage subsidy, compare plan 3 ($2,400 guarantee for a family of four and a 50 percent tax rate) with plan 15 (the subsidy equals 50 percent of the difference between the individual's wage and $2 up to 2,080 hours). Both plans cost about $8.6 billion. The NIT (plan 3) lowers hours worked by 12 percent while the *WS* (plan 15) reduces work by only 1.7 percent. However, only 39 percent of those receiving the *WS* were initially below the poverty line compared to 56 percent for the NIT; 33 percent of the wage subsidies go to those with incomes above the NIT break-even income level ($4,800 for a family of four) compared to only 1 percent for the NIT. The wage subsidy induces more work but is less efficient in transferring income to the poor.

As the maximum hours increase, the hours reduction for those working more than the original maximum hours decreases. The budget cost and real cost increase. The distributional effect of the maximum hours changes is almost nonexistent because those with low wage rates tend to work long hours. This cancels out the tendency for a relaxation of the hours restrictions to increase the benefits of those with higher incomes. Elimination of the restriction entirely results in an increase in hours of work as can be seen in plan 19. If plan 18 and plan 19 are compared it can be seen that the more favorable work incentives of the plan without an hours restriction are obtained at the expense of a 13-percent increase in the budget cost and a doubling of the real cost.

A reduction in the tax on nonwage income increases the number of recipients because individuals will choose the subsidy regardless of their other income (compare plan 18 and plan 20). The budget cost also increases when the tax is lowered, and a higher proportion of the benefits are paid to higher income individuals. A 100-percent tax on nonwage income is effective in meeting most of the objectives described above, but it may discourage saving and private transfers.

If either the husband or wife or both are allowed to qualify for a wage subsidy, the cost is greatly increased. By comparing plan 14 with plan 13, it can be seen that by allowing the wife of an employed man to qualify for a subsidy the cost is increased by 52 percent and the number of filing units increases by 26 percent. In addition the real cost is increased and 64 percent of the benefits paid to two wage-earner families go to families with incomes above $7,000. The overall percentage reduction in hours worked is also greater. The seemingly minor provision that only the head of the family can receive a subsidy is of major importance. One might want to treat husband and wife equally, but this allows wives of men with high wage rates and incomes to receive benefits. This emphasizes a central problem associated with the wage subsidy. Since it does not include an income test, it is not effective in concentrating benefits on low-income families. If we add family stability as an additional objective, equal treatment of husband and wife might be deemed necessary in order to prevent families from separating. Again the cost minimizing objective conflicts with other reasonable social goals.

A program that would offer a wage subsidy to those who are categorized as able to work and a guaranteed income for all others would have little impact on those already in the labor force. This is in fact the aim of such a program. Suppose that plan 19 is introduced with a guaranteed income (plan 5) for those with children under 6 and no spouse present.[23] The cost of this combination plan exceeds the cost of plan 19 by $692 million, but only 39,000 more filing units are added. The decline in hours worked for the not married, spouse-present group is 7.6 percent as compared with 3.3 percent with plan 19. Only 3 percent of the filing units (559,000) would receive the guaranteed income. Of course there would be a large group outside of the labor force who would qualify. The big advantage of combining two such plans is that more adequate income is provided for those groups not likely to be in the labor force. For instance, those over 65 could be added as guaranteed income recipients. A basic deficiency of the wage subsidy—low benefits to low-income groups in the labor force—remains. Furthermore these categorical programs may deviate from the goal of horizontal equity.

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[23] They were assumed to be excluded from the wage subsidy.

TABLE 3.—*Earnings subsidy, 25 percent subsidy up to $3,000, 50 percent tax rate on earnings thereafter, no tax on nonwage income, age 25 and over and in the labor force, 1966*

Plan number	Group [1]	Thousands of filing units	Gross budget cost (millions)		Percent decline in work hours (increase) [2]	Percent of recipients		Recipients below poverty line		Percent of benefits to those initially [3]					Real cost (millions)
			If program caused no work reduction	Including cost of work reduction		Below $3,000	Above $3,000	Before program	After program	Below guarantee	Guarantee to break-even	Above break-even	Below $3,000	Above or equal $3,000	
22	M & F, NMSP	9,463	$3,274	$3,975	4.4	76	24	35	28	7	15	78	74	26	$761
	M, MSP, SNW	3,615	1,207	1,520	6.8	59	41	34	28	17	44	39	63	37	310
	M & F, MSP, SW	2,623	1,008	966	(.7) 1.2	59	41	38	30	19	44	37	64	36	180
	Total	15,701	5,489	6,461	3.7	69	31	35	28	11	27	62	70	30	1,251

[1] M=male, F=female, MSP=married-spouse-present, NMSP=not married-spouse-present, SW=wife in labor force, SNW=wife not in labor force.

[2] Where applicable: Husband's response; wife's response.

[3] The guarantee and break-even levels are for plan 3.

The earnings subsidy outlined earlier was also simulated. As shown in table 3, the plan has a relatively low budget cost. When the earnings subsidy is compared with a wage subsidy of similar budget cost, plan 21, it can be seen that the earnings subsidy produces fewer disincentives but has a substantially higher real cost. The improved incentives of the earnings subsidy occur because of differences in the groups receiving benefits. For reasons given above one expects an earnings subsidy to offer greater disincentives. The higher real cost occurs because of the 50 percent tax on earnings above $3,000. The earnings subsidy also pays out a lower percentage of benefits to those below the plan 3 guarantee level and a much larger percentage to those with higher incomes. As compared to a wage subsidy the earnings subsidy is inferior with respect to costs and impact on the poorest individuals, but it may encourage increases in the wage rate. Although it did not show up in these simulations, the earnings subsidy will probably be inferior to a wage subsidy with respect to incentives.

When compared to a negative income tax of comparable cost (plan 4), the earnings subsidy has fewer disincentives to work, but it is significantly inferior in terms of providing benefits to the poor. Only 35 percent are initially below the poverty line as compared to 78 percent under plan 4. Seventy-one percent of the benefits go to those with incomes less than the guarantee level under the NIT plan (No. 4), while with the earnings subsidy only 11 percent of the benefits go to this group. The real cost of the earnings subsidy is also higher.

Changes in labor force participation that might result from these transfer programs were not simulated because of estimation difficulties. It should be remembered that negative income taxes will tend to reduce labor force participation for those not already receiving welfare, while wage and earnings subsidies can only increase labor force participation. This could improve the relative incentive effects of the wage subsidy.

A vexing problem that is extremely difficult to escape is the proliferation of social programs which have marginal taxes on income. If the cost of medical care, housing, and so forth, goes up as income increases, the marginal tax on income quickly approaches or exceeds 100 percent after addition of a negative income tax.[24] This problem negates the advantage of the negative income tax over the guaranteed income. An advantage of the wage subsidy is that the additional marginal taxes from other programs would be less likely to lower the net return to work to zero. In this sense, the wage subsidy would be more compatible with the existing programs than a negative income tax.

IV. CONCLUSION

The simulations highlight the inherent conflicts between the objectives that were described. Programs which minimize the reductions in work effort tend to be inefficient in their impact on poverty. Programs with ample benefits for those with low incomes tend to be extremely costly. A decrease in the marginal tax rate for a negative income tax increases the budget cost and gives more benefits to those above the poverty line. Conversely, reductions in benefits to those with higher incomes through an increased tax rate reduce the incentives to work and increase the real cost.

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[24] Robert I. Lerman, "Incentive Effects in Public Income Transfer Programs," in *Studies in Public Welfare, Paper No. 4, Income Transfer Programs: How They Tax the Poor*, prepared for the Subcommittee on Fiscal Policy, Joint Economic Committee (Washington, D.C.: U.S. Government Printing Office, 1972), pp.1–78.

Restrictions on the amount of per hour wage subsidy going to those with higher wage rates reduce the incentives to increase one's wage rate. Equal treatment of working wives greatly increases the cost of a wage subsidy. An increase in the tax on nonwage income increases work effort, lowers the budget cost, and makes the program more efficient in transferring income to the poor. However, it may inhibit saving and private transfers.

The central point of this paper is that there is no way of simultaneously meeting all of these objectives for income maintenance programs. The simulations have indicated the rate at which one can trade off a particular goal for another. It is up to the political process to determine the most desirable program.

SUPPLEMENTARY MATERIALS

1. The Family Assistance Plan

A variation on the negative income tax, the family assistance plan, was simulated. The family assistance plan as originally proposed featured a $1,600 guarantee for families with children, a 50-percent tax rate on earnings over $720 per year, and a 100 percent tax rate on unearned income. State supplements, food stamps, and other existing programs would have raised the tax rate considerably. The version of the family assistance plan simulated here is a later version referred to as H.R. 1.[25]

It eliminates the food stamp program for those receiving family assistance, raises the guarantee to $2,400 for a family of four, and raises the tax rate to 67 percent. The per capita guarantee declines as the family size increases, reaching a family maximum of $3,600 (for eight or more).[26] The first $720 of earned income is exempted as is $120 of irregular earned income and $240 of irregular nonwage income. The tax rate on additional nonwage income is 100 percent. The administrators of the act are to specify limits on student earnings. All families with children under 18 or students under 22 are eligible for the program. The bill also includes a number of features designed

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[25] U.S. Congress, *Social Security Amendments of 1971: Report of the Committee on Ways and Means on H.R. 1*, H.R. 92-231, 92d Cong., 1st sess. (Washington, D.C.: U.S. Government Printing Office, 1971).

[26] The guarantee depends on total family size:

Family size:	*Guarantee*
1	$800
2	1, 600
3	2, 000
4	2, 400
5	2, 800
6	3, 100
7	3, 400
8 or more	3, 600

to insure that those not aged or caring for infants find work. Those able to work are to receive benefits, calculated on the same basis, from the opportunities for families program. These provisions were not considered in the simulation of this plan. It was assumed that the irregular income provisions would be liberally interpreted, allowing an $840 earnings exemption and a $240 nonwage income exemption.

The simulation differs from the bill in several ways: (1) Students between ages 18 and 22 are not counted as children in the simulation: (2) alimony and support are treated as other nonwage income (100 percent tax) instead of being taxed at a 67 percent rate; (3) costs of child care are not deducted from earnings; (4) no account is taken of assets; and (5) there is no separate program for the blind, aged, and disabled. The aggregate costs presented here are for 1966. Changing income, employment conditions and population can alter the cost considerably. Variations in H.R. 1 which extend the plan to families without children and raise the guarantee by 33 percent ($3,200 for a family of four) were also simulated.

2. *Simulation of the Wage Subsidy*

The techniques used in the simulation of the effects of the negative income tax have been explained elsewhere.[27] The earnings subsidy simulation is analogous to the simulation of a negative income tax with an earnings exemption.[28] In this section the wage subsidy simulation procedure is explained. For a wage subsidy with no maximum hours there is no problem. The wage for each qualifying individual is changed and the response is calculated. The simulation becomes slightly more complicated when maximum hours and a tax on nonwage income are added.

Figure A-1 is a standard leisure-income diagram. The individual has received a wage increase as a result of a wage subsidy. In the absence of a maximum hours restriction he chooses point B. With an hours maximum ($HMAX$) the maximum subsidy occurs at point D, and B is unobtainable. In the first stage of the simulation the wage is increased for all who qualify. For those falling on points such as B, the supply is predicted again with nonwage income equal to $HMAX/(W_s-W)=FC$ and the wage equal to the actual wage rate. If he falls on the segment DE the procedure is finished. If he fails on segment CD, his preferred position must be at D where the number of subsidized hours is at a maximum.

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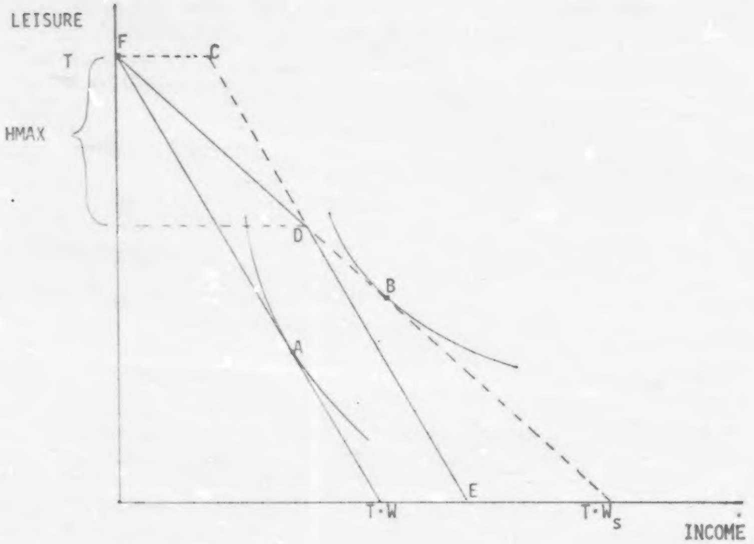
[27] Rea, "The Supply of Labor and the Incentive Effects of Income Maintenance Programs," op. cit.

[28] Ibid., p. 192-194.

FIGURE A-1



If nonwage income is taxed at a rate r_{NW}, the simulation is more complicated. As shown in figure A–2 no one with nonwage income that exceeds $(W_S-W)\cdot HMAX+(l-r_{NW})\cdot Y_{NW}$ would choose to receive a subsidy. The original budget line for such an individual would dominate the wage subsidy program. In figure A–3 the individual with

$$Y_{NW}<(HMAX\cdot(W_S-W)+(l-r_{NW})\cdot Y_{NW})$$

may wish to take advantage of the subsidy. If he is initially along segment *BF*, he will definitely want the subsidy. If he is initially along segment *AB*, he may want to increase his work and move to a position on *BDE*. His hours of work under the subsidy ($\hat{H}_{WS}$) are first predicted under the procedure described above. If he is predicted to fall on segment *DE*, by revealed preference he definitely prefers the wage subsidy. If he was predicted to fall on segment *BD* or on *CD* and was assigned point *D*, he will prefer to take advantage of the subsidy if [29]

$$r_{NW}\cdot Y_{NW}<\hat{H}_{WS}\cdot(W_S-W)-.5\cdot(W_S-W)^2\cdot\left(\frac{\partial\ \text{Leisure}}{\partial W}\right)_{\overline{U}}$$

This follows from Hicks' compensating variation in income.[30]

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[29] W_S in the inequality is set equal to $(W_S+W)/2$ if the person was assigned point D.

[30] Rea, "Incentive Effects of Alternative Negative Income Tax Plans," op. cit.

FIGURE A-2

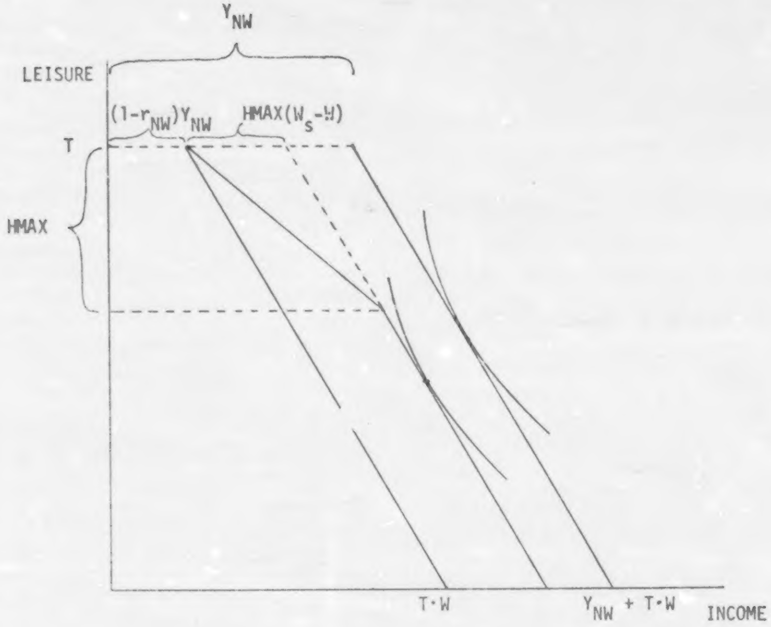
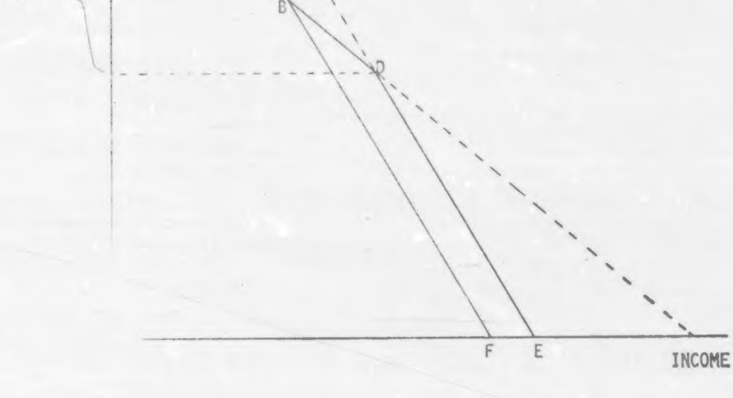

Y_{NW}
LEISURE
$(1-r_{NW})Y_{NW}$
$HMAX(W_s-W)$
T
HMAX
T·W
Y_{NW} + T·W
INCOME

FIGURE A-3


LEISURE
Y_{NW}
$(1-r_{NW})Y_{NW}$
A
C
HMAX
B
D
F
E
INCOME

AN EXAMINATION OF RECENT CROSS-SECTIONAL EVIDENCE ON LABOR FORCE RESPONSE TO INCOME MAINTENANCE LEGISLATION

By GLEN G. CAIN *and* HAROLD W. WATTS

Any income maintenance legislation may be described by (i) the amount of an income guarantee—the transfer payment the family unit would receive if it had no other income—and (ii) the rate at which this guarantee is reduced as the family receives income from other sources. (The rate of reduction need not be constant.) Earnings are the main source of the nontransfer income of low-income families eligible for Government benefits, and the benefit reduction has the effect of lowering the earnings rate (or wage rate) for the working members of such families. A major question facing policymakers is: How would such a decline in their earnings rate affect the amount of work performed by beneficiaries of income transfers?

The following paper discusses, in the context of seven major, recently published, pieces of research: [1] (1) the methodological problems involved in trying to answer this question by means of estimating the effects on labor supply of variations in income and wage rates recorded in cross-section data; and (2) different empirical estimates of the income and substitution effects of such a program on labor supply.

I. BACKGROUND

Efforts to measure the influence of income and prices upon economic behavior are nearly as old as the science of economics. The origins of econometric research are often traced to the famous studies of Ernst Engel more than 100 years ago of the effects of income on spending patterns of families.[2] The study of price (or substitution) effects is as ancient as the question: "What will the effect be of a change in taxes on the quantity purchased of the taxed item?"

Lionel Robbins' classic article [3] on the supply of labor in terms of the demand for leisure has led to the fruitful approach of analyzing the effect of income and prices (wage rates) on the supply of labor. He divided the discretionary time of an individual into lesisure and work activities and noted that an increase in wage rates would raise the price of leisure relative to time spent at work. Because of the economic axiom that a rational individual will shift his consumption toward goods whose relative price has fallen (in this case wage goods obtained from working) and away from goods whose relative price has risen (in this case leisure), under "other-things-equal" conditions the presumption is that the substitution effect of wages on leisure is negative. Robbins also noted that the increase in wages will increase income, permitting the individual to buy more of all goods. Since leisure is a normal good, the rise in income is expected to increase the purchase of leisure, leading to a decrease in time spent at work. Thus wage changes, like all price changes, set in motion both substitution and income effects; but here the effects are of opposite signs. These theoretical considerations are in the background of the longstanding issue of whether a tax on earnings, particularly a progressive income tax, has any effect on work effort—and since labor constitutes 75 to 80 percent of the national income of modern nations like the United States, any analysis of labor supply is of more than academic interest.

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[1] See bibliography.

[2] Two interesting accounts of the work of Engel and other precursors of modern econometrics are: George J. Stigler, "The Early History of Empirical Studies of Consumer Behavior," *Journal of Political Economy*, v. XLII, April 1954; and H. S. Houthakker, "An International Comparison of Consumer Expenditure Patterns, Commemorating the Centenary of Engel's Law," *Econometrica*, v. 25, October 1957, pp. 532–551.

[3] Lionel Robbins, "On the Elasticity of Demand for Income in Terms of Effort," *Economica*, vol. 10, June 1930, pp. 123–129.

It is not, however, the positive income tax that is most hotly debated today regarding labor supply effects. The positive income tax is no longer widely believed to have serious consequences for work effort—although one may rightly question the evidence for this assumption. It is now those who face the lowest positive income tax rates, or even no income tax at all, who are the focus of the greatest interest and controversy.

Current reappraisals of the welfare system have heightened scientific and public interest in the effect of income maintenance laws on the work behavior of poor people. For several years welfare programs of some States have provided income guarantees larger than the earnings of the poorest among the working poor. The welfare system also generated sharp disincentives to work in the form of high implicit tax rates (explicit benefit-loss rates) on earned income.[4] These features, along with other factors such as the growth in female-headed households, have contributed to the rapid growth of welfare caseloads and costs. Disincentives to work are affecting more people and the injustice of denying cash benefits to intact families of working fathers has become more obvious. Public resentment is so widespread that the President of the United States has referred to the system as a "colossal failure."

A number of reforms have been proposed to replace the existing categorical welfare programs with a comprehensive income maintenance program covering the working poor as well as the nonworking poor. These proposals, exemplified by the family assistance plan (FAP), have increased the importance to policymakers of the labor supply response issue on two counts. First, these reform plans all would increase substantially the number of families receiving income maintenance payments. Second, the bulk of these additional families have at least one family member with an attachment to the labor force, as evidenced by their current employment.

The public fears that massive numbers of people will quit work in favor of living on the dole. Whether or not that fear is well-founded—and it does not seem to be—there are at least three ways in which a less drastic work response affects the evaluation of an income maintenance policy. First of all, there is the effect on real output—if the aided families produce less, and no one else produces more, there will be less real product (although more leisure) to be distributed altogether. Second, a change in labor supply, at any given level of demand, may reduce earnings and consequently increase the amount of income-related benefits that must be paid. Third, and closely related to the second, work and earnings reductions are only partially offset by benefit increases, so that the net increase in the spending income of benefit families is smaller than the benefit itself. In short, the response is crucially related to the real aggregate effects and to both sides of the cost/benefit criterion. It must be noted that there are offsets to any reduction in real output. First, some of the time withdrawn may be used for education or other human investment and hence augment future output. Second, nonmarket production (and other leisure activities) will serve as a substitute for paid employment, reducing the impact of any comprehensive measure of welfare.

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[4] Note that both the income effect from the positive transfer payments and the substitution effect from the high (implicit) tax rate on earnings operate to reduce the labor supply of the affected population. This situation differs from that of the nonwelfare population, since higher tax rates on their earnings do not have the same offset in the form of transfer payments which increase the household's income.

The so-called working poor have in the past received almost no income transfers (other than food stamps) from the American welfare system—such transfers have predominantly been paid to nonworking categories of the population. In addition, the working poor have faced relatively low income tax rates. Both conditions would change dramatically under a negative income tax system. For example, under the family assistance plan, a working poor family of four would be eligible to receive $2,400 a year in cash payments if it received no other income; and the tax rate facing such a family would range from 60 to 90 percent over varying ranges of earned income levels.[5]

Thus, even if we think that experience with the positive income tax indicates that taxes on earnings have only a small effect on labor supply, we must recognize that we can by no means generalize from those relatively low positive tax rates to the combination of high tax rates and direct income transfer payments proposed for negative income tax legislation. Not only are the negative income tax rates much higher, but they also are harder to escape through legal loopholes; further, because work conditions for the poor are less satisfying, we cannot expect from them the same commitment to continued work as from those whose jobs pay well, are pleasant, and impart social prestige.

Our experience to date, therefore, gives us little guidance for assessing the economic and social effects of income maintenance laws—in particular the effect on work effort. Beginning with the poor law debates in England—and much can be learned from these debates—there is substantial literature on this question.[6] But little empirical work has been done, and empirical evidence (as has always been recognized) is very much needed if valid conclusions and policy guidelines are to be forthcoming.

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[5] See D. Lee Bawden, Glen G. Cain, and Leonard J. Hausman, "The Family Assistance Plan: Analysis and Evaluation," *Public Policy*, spring 1971, vol. XIX, pp. 323-354.

[6] A review of this literature would be rewarding. One instructive difference between those debates and current discussion is the longrun perspective with which the classical and neoclassical economists analyzed the poor laws. The current discussions usually deal with a timespan that is only long enough to permit adjustment of the current adult population to the new regime of income transfers and wage rates. The former debates, perhaps because of the concern with which the classical economists viewed the effect of wages on population growth, encompassed the consequences of income maintenance laws over many generations. From this perspective, Alfred Marshall called attention to the possible longrun beneficial effects of income supplements on work and earnings, on the grounds that children from poor families would be expected to be better educated, in better health, and in other ways more productive upon reaching adulthood. See, for example, the views of both Malthus and Marshall as they are reported in D. V. Glass, ed., *Introduction to Malthus*, London, Watts & Co., 1953, especially pp. 62-63 and 177-192.

Recent advances in data gathering, theoretical tools, and statistical techniques have made data collection and analysis more feasible. And the research discussed below has endeavored to address these questions empirically, by using the available information on labor supply, wage rates and nonwage income to estimate the quantitative effects of income and wage rates on labor supply. They have been able to place bounds on the relevant parameters, and they have devised ways of translating this information to the case of poor families under a negative income tax plan.

The seven studies discussed below have been published together in the book cited in the bibliography. All the papers use the 1967 Survey of Economic Opportunity as their data base with the exception of Fleisher et al. (7). David Greenberg and Marvin Kosters (1) have focused on male heads in families with incomes of $15,000 and under. A major distinguishing feature of this study is an effort to control for differences in individual preferences that could yield cross-sectional relationships that might be misleading as to intertemporal responses. Robert Hall (2) uses a more comprehensive sample from the SEO, treating a complete classification of adults and teenagers. He does, however, limit consideration to the low-income (more properly low-wage) strata living in the 12 largest standard metropolitan statistical areas. Michael Boskin's paper (3) is similar to Hall's, but uses a larger and less homogeneous sample. It does, however, differ from all the other studies in that it first analyzes the labor-force participation question as a binary one, and then goes on to analyze the quantities of labor supplied. C. Russell Hill (4) pays primary attention to the male family head, and aims at further homogeneity by limiting the sample to heads of husband-wife families with no other adults and who have incomes below the official poverty lines.

Irwin Garfinkel (5) examines the sensitivity of typical labor-supply coefficients to various choices that have to be made regarding sample selection and model specifications of any empirical study, and finds that a substantial range of estimates can be obtained by varying these specifications. Orley Ashenfelter and James Heckman (6) put their major methodological emphasis on the restrictions provided by classical consumer-choice theory. Fleisher et al. (7) use a new and promising set of panel data,[7] focusing primarily on the mediation of net worth in the labor supply choice—more specifically on how disequilibrium levels of accumulated wealth bear on the labor-supply choices of older workers (aged 45–59). Their use of measures of assets and wealth invites comparison with the Greenberg-Kosters study (1), where the same variables are used as preference indicators rather than as direct arguments of the labor supply function.

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[7] The data come from a 5-year study (directed by Prof. Herbert Parnes of Ohio State University) of the labor-market experience, characteristics and work attitudes of four groups—men 45–59 years old, women 30–44 years old, and young men and young women 14–24 years old. They use the first two interview waves, administered in June 1966 and June 1967 to 3,500 white and 1,500 black U.S. noninstitutional, civilian males aged 45–59.

Before we turn to our substantive discussion of the empirical estimates that have been made and the methodological problems encountered, one point should be noted. An important empirical datum that faces economists is that there has been a longrun decrease in the amount of time spent at market work by United States males. This decline, however, has been largely offset by increased work on the part of married women. Since 1950, for instance, the proportion of women with children who are in the labor force has increased from 1 in 10 to 1 in 3. The work behavior of males, then, explains only part (and an increasingly smaller part) of the trends in the household supply of labor to the market. This has important implications for any study of the effects of income maintenance laws or other tax laws on work behavior, because the *family* labor supply becomes more discretionary, more flexible—in a word, more responsive to wage and income effects.

II. Quantitative Estimates of Income and Substitution Effects

A summary listing of estimates of income and substitution parameters, calculated from these seven studies and certain others, is presented in table 1 (for males) and table 2 (for females). We report substitution elasticities and total income elasticities in these tables—measures which have several advantages compared with other ways in which the parameters could be displayed. The use of total income elasticities converts all income coefficients in labor supply equations to comparable units independent of the size of income components used to measure the income variable. (See the explanatory note defining the total income elasticity in table 1.) It adjusts, in other words, for the use of nonlabor income as compared with total family income. Since the negative of the total income elasticity measures the percentage change in consumption of leisure with respect to the percentage change in total income, one can readily determine if it is negative—which it should be if leisure is a noninferior good; and if it is less than one in absolute value—which it should be if income (or wage goods) is noninferior. The substitution elasticity (which is simply the wage elasticity minus the total income elasticity) is expected to be positive on the basis of the postulates of economic theory, and this theoretical expectation can be observed directly when the substitution elasticity, rather than the wage elasticity, is displayed. Obviously, the wage elasticity, which expresses change in labor supply with respect to the uncompensated percentage change in wages, can be determined simply by adding the substitution and total income elasticities. A total income elasticity which is larger in absolute value than the substitution elasticity is confirmation of the backward-bending supply curve of labor.

TABLE 1.—*Various point estimates of income and substitution parameters for males*

Author	Data source—Year	Race—Marital status—Age group	Substitution elasticity [1]	Total-income elasticity [2]
Ashenfelter and Heckman.[3]	SEO—1967	Husbands 25 to 64, wives not working.	.12	−.27.
	SMSA aggregates in Census—1960.	Male labor-force participation rates for SMSA's.	.06	−.06.
Boskin [4]	SEO—1967	Husbands 20 to 59	.10 (white)	−.17 (white).
			<0 (black)	−.06 (black) estimated.
Cohen, Rea, Lerman.[5]	CPS—1967	Males 22 to 54	Negative	−.08 to +.03.
Fleisher, Parsons, and Porter.[6]	National Longitudinal Survey, 1966–67.	Urban husbands 45 to 59:		
		"Full" sample	.04	−.23.
		Excluding men who received work-conditioned nonemployment income.	−.19	−.08.
Garfinkel [7]	SEO—1967	Able-bodied husbands 25 to 61 in labor force.	Near 0 (sometimes positive and sometimes negative).	Near 0 (sometimes positive and sometimes negative).
Greenberg and Kosters.[8]	SEO—1967	Married males in labor force <62; income <$15,000.	.02 to +.20 (authors' preference: 20).	−.15 to −.34 (authors' preference: −.29).
Hall [9]	SEO—1967	Husbands 20 to 59 with predicted wage per hour <$3.00.	−.20 to +.39 Weighted ave.= .06 (white).	−.24 to −.51 (white).
			−.68 to +.25 Weighted ave.= −.10 (black).	−.12 to −.28 (black).
Hill [10]	SEO—1967	Males 25 to 54:		
		Income below poverty line	.47 (white)	−.68 (white).
			.27 (black)	−.35 (black).
		Income above poverty line	.52 (white)	−.86 (white).
			.56 (black)	−.88 (black).
Kalachek and Raines.[11]	CPS—1966	Males 24 to 61, income <$8,500	.86 (white) .96 (nonwhite)	−.31 to −.33.

TABLE 1.—*Various point estimates of income and substitution parameters for males*—Continued

Author	Data source—Year	Race—Marital status—Age group	Substitution elasticity [1]	Total-income elasticity [2]
Parker [12]	SEO—1967	Males with children:		
		Below poverty line	0 (full year, full time)	−.04.
			.34 (full year, part time)	−.34.
		Above poverty line	−.05 (full year, full time)	.001 to −.15.
			.27 (full year, part time)	
Rosen and Welch [13]	.001 Sample in Census—1960.	Employed husbands 25 to 65, income <$10,000.	(range: negative to .6) ave.=.2 to .3.	.001 to −.15.
Tella, Tella, and Green.[14]	SEO—1967	Male heads, 18 to 64, wage per hour <$3.00.	.16 to .45 (authors' preference: .16 to .18).	−.11 to −.38 (authors' preference −.11 to −.16).

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[1] When the substitution elasticity is evaluated at the means, it is defined as $(\partial L^s/\partial W)(\overline{W}/\overline{L})$, where W=wage rate and L=labor supply (for example, hours worked per year). The superscript, s, on $\partial L^s/\partial W$ distinguishes the compensated effect of a change in wages from the uncompensated effect (written without the superscript). The two are related as follows: (1) $\partial L^s/\partial W=\partial L/\partial W-(\partial L/\partial Y)(L_o)$ where $\partial L/\partial Y$ is the income effect (or regression coefficient in a linear and additive model) and L_o is the "prechange" or equilibrium value of labor supply. Income$=Y=WL_o+Y_n$ where Y_n=nonlabor income. The substitution elasticity is obtained by multiplying (1) by $\overline{W}/\overline{L}$: $(\partial L^s/\partial W)(\overline{W}/\overline{L})=\eta_{\omega^s}=\eta_\omega-(\partial L/\partial Y)(\overline{W}/\overline{L})(L_o)$. It is convenient to express the last term as a weighted income elasticity (see [2]). Thus, $\eta_{\omega^s}=\eta_\omega-\eta_y\overline{W}(L_o/\overline{Y})$. If the income effect is measured as $\partial L/\partial Y_n$, then the substitution elasticity is defined as: $\eta_{\omega^s}=\eta_\omega-\eta_{yn}(\overline{W}L_o/Y_n)$ where $\eta_{yn}=(\partial L/\partial \overline{Y}_n)(\overline{Y}_n/L)$.

[2] The total-income elasticity is defined as $\eta_y(\overline{W}L_o/Y)=(\partial L/\partial Y)(\overline{Y}/\overline{L})(\overline{W}L_o/Y)$, which equals the conventionally measured income elasticity weighted by the fraction that earnings is of the income variable used. By the conventional assumption that $L_o=\overline{L}$ and $Y=\overline{Y}$, the total-income elasticity reduces to $(\partial L/\partial Y)\overline{W}$. The total-income elasticity is the same whether measured as $\eta_{yn}(WL_o/Y_n)$ or $\eta_y(WL_o/Y)$, whereas the conventionally defined income elasticity, measured as η_Y or η_{yn} will differ depending on the sizes of the following two fractions: $\overline{Y}/\overline{W}=[(\overline{WL}+\overline{Y}_n)/\overline{W}]$ and $\overline{Y}_n/\overline{W}$. The use of a total-income elasticity serves to make the income-parameter estimates in the various studies more comparable and permits a direct comparison with the observed uncompensated wage elasticity to determine whether the sign of the unobserved substitution elasticity is positive.

[3] Orley Ashenfelter and James Heckman: p. 277. The total-income elasticity is estimated as $W(\partial L/\partial Y)$. The substitution and income elasticities estimated with SMSA aggregative data from the 1960 Census are reported in another paper of theirs, "The Estimation of Income and Substitution Effects in a Model of Family Labor Supply," *Econometrica* (forthcoming).

[4] Michael J. Boskin: Table 4.4, p. 177. The total-income elasticity is the wage elasticity minus the substitution elasticity for white prime-age husbands. For black prime-age husbands the estimated substitution elasticity appears to be negative. The total-income elasticity is estimated by Cain and Watts to be about −.068 to −.072 based on the formula $(\partial h/\partial Y)W$. Boskin reported $\partial h/\partial Y=.029$ and the average wage for black husbands is assumed to be $2.00–$2.50 per hour. Boskin, in his paper, reports a positive substitution elasticity after he constrains the income and wage effects to be equal in his regression of hours worked.

[5] Malcolm S. Cohen, Samuel A. Rea, Jr., and Robert I. Lerman, "A Micro Model of Labor Supply," (Washington, D.C.: U.S. Government Printing Office, 1970) pp. 50–55. (U.S. Department of Labor, Bureau of Labor Statistics Staff Paper 4). The income elasticities are defined as $(\Delta h/\Delta Y)(Y/h)$, and are computed for two wage groups of males (wages = 0–$.99 and $1.00–$2.49 per hour) and three income changes from four income groups (0–$499, $500–$1,499, $1,500–$3,499, and $3,500+).

[6] Belton M. Fleisher, Donald O. Parsons, and Richard D. Porter: Table 8D.1, line number 12, for the first set of income and substitution elasticities. This regression was selected because observations with work-conditioned nonemployment income (like welfare payments and Unemployment Insurance benefits) were excluded. The sample size was 866. The second set of elasticities was derived from line number 12A, Table 8D.1—with the same data except that the men who received any work-conditioned nonemployment income were excluded. The sample size was 692.

[7] Irwin Garfinkel: pp. 211–213. The elasticities are implied in the cited discussion. The uncompensated wage elasticities are between −.008 and −.02 in the hours regression and between +.016 and −.01 in the full-time, part-time work regression. The income effects in table 6.1 are small positive and small negative numbers that are statistically insignificantly different from zero. In regression equation number 28, in app. 6 (using the basic sample), the income effect is positive but is insignificantly different from zero. The uncompensated wage effect is derived from a quadratic wage function and is positive for wage rates less than $2.54 and negative for wage rates over $2.54 per hour. Thus, for low-wage workers the substitution effect would be positive.

[8] David H. Greenberg and Marvin Kosters: Tables 2.6, 2.11, and 2.12. The authors suggest .20 as an "intermediate estimate" of the substitution elasticity (p. 50) and −.10 as an intermediate income slope effect—which implies an income elasticity of around −.29. The income elasticities are obtained from the elasticities of nonlabor income weighted by the ratio of mean earnings over mean nonlabor income.

[9] Robert E. Hall: Table 3.5, p. 133. The elasticities are based on this table and are computed by the arc-elasticity formula. For example, the wage (or substitution) elasticity is: $[L_1-L_2/\frac{1}{2}(L_1+L_2)]\ [\frac{1}{2}(W_1+W_2)/W_1-W_2]$. In computing the wage elasticity, the values for L_1 and L_2 are for adjacent cells and are obtained for an assumed representative group of husbands, age 20 to 59, whose whole income is $3,750-$4,500, with two adults in the family, and with both preschool and school-age children present.

[10] C. Russell Hill: Table 5.5, p. 202. For the poor, the total-income elasticities are obtained from the elasticities of transfer-payment income weighted by the ratio of mean earnings over mean transfer-payment income. For the nonpoor, the total-income elasticity is based on coefficients and amounts for wealth income and transfer-payment income.

[11] Edward D. Kalachek and Fredric Q. Raines, "Labor Supply of Lower Income Workers," *Technical Reports*, The President's Commission on Income Maintenance Programs (Washington, D.C.: U.S. Government Printing Office, 1970): 179.

[12] Carl D. Parker, "The Determinants of Hours of Work of Low-Income Family Heads: A Statistical Analysis," (Ph. D. dissertation, Oklahoma State University, July 1971): 102–104 and 123–124. For male, full-year, full-time workers below the poverty line, the reported wage elasticity is −.043 and the income effect ($\partial L/\partial Y$) is −.0227. We have estimated the average wage rate to be $1.80 and computed a total income elasticity of −.40. Parker reports a zero income elasticity and a substitution elasticity equal to −.043. All other elasticities are as reported by Parker. The regression results reported in the table are those using all family income (except the head's own earnings) as the income variable. Another set of results are reported in which income is only nonlabor income (including transfer payments).

[13] Sherwin Rosen and Finis Welch, "Labor Supply and Income Redistribution," *The Review of Economics and Statistics* 53 (August 1971).

[14] Alfred Tella, Dorothy Tella, and Christopher Green, "The Hours of Work and Family Income Response to Negative Income Tax Plans" (Kalamazoo, Mich.: W. E. Upjohn Institute for Employment Research, 1971). The authors compute two sets of substitution elasticities: (1) the sum of the estimated uncompensated wage elasticity and the total-income elasticity, and (2) a "direct method," by comparing differences in hours worked by different wage groups with "similar" amounts of total income. For wives (see table 9.2, p. 336) only the "direct method" elasticities are reported. Elasticities are usually reported as averages over several wage classes—for example, for groups whose hourly wage rates are *less* than $1.75 and *less* than $3.00, and for groups with and without persons who are not in the labor force.

Source: Unless otherwise indicated, see Glen Cain and Harold Watts (editors), *Income Maintenance and Labor Supply: Econometric Studies*, Institute for Research on Poverty Monograph Series, Markham Press, Chicago, 1973.

TABLE 2.—*Various point estimates of income and substitution parameters for females*

Author	Data source-year	Marital status-age group	Substitution elasticity	Total-income elasticity
Ashenfelter and Heckman.[1]	SMSA aggregates Census, 1950, 1960.	All married women, labor-force participation rates for SMSA's.	1.2	−.28.
Boskin [2]	SEO–1967	Wives 21 to 59	.12 (white)	−.06 (white).
			.58 (black)	−.07 (black).
Hall [3]	SEO–1967	Wives 21 to 59	1.6 to 2.7. Weighted average = 2.5 (white).	Average = 2.1 (white).
			−5.9 to 3.7. Weighted average = .26 (black).	Average = 1.4 (black).
Kalachek and Raines.[4]	CPS–1966	Females 21 to 64 low-income families.	.76 to .85 (white)	
			.14 to .28 (nonwhite)	−.41 to −.75.
Parker [5]	SEO–1967	Female heads, poor and near poor, with children:		
		Below poverty line	−.11 (full year, full time)	0.
			.06 (full year, part time)	−.21.
		Above poverty line	−.12 (full year, full time)	0.
			.40 (full year, part time)	−.46.
Tella, Tella, and Green.[6]	SEO–1967	Female heads 18 to 64	.11 to 1.95 (authors' preference: .11 to 1.14).	−.22 to −.81.
		Wives, wage per hour <$3.00	.23 to .35	−.07 to −.38.

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[1] Ashenfelter and Heckman: See footnote 3 in table 1.

[2] Boskin: Table 4.4, p. 177 in *Income Maintenance and Labor Supply: Econometric Studies.* The total-income elasticities are the wage elasticities minus the substitution elasticities.

[3] Hall: These elasticities are based on the arc-elasticity formula, $[L_1 - L_2/\frac{1}{2}(L_1+L_2)]$ $[\frac{1}{2}(W_1+W_2)/W_1-W_2]$, in which the adjacent cells are the sources of the L_1, L_2, etc. values. In computing the elasticities, the values of L_1 and L_2 are obtained for wives whose whole income falls in the $3,750–$4,500 class, age 20 to 59, 2 adults in the family, with both preschool and school-age children present, and a predicted wage in the $1.75–$2.00 class. See his article in *Income Maintenance and Labor Supply, Econometric Studies.*

[4] Kalachek and Raines: See footnote 11 in table 1.

[5] Parker: See footnote 12 in table 1.

[6] Tella, Tella, and Green: See footnote 14 in table 1.

Tables 1 and 2 show a consensus in support of the economist's presumptions regarding the signs of the income and substitution effects. There is, it appears, every reason to believe that positive income transfers will exert a positive influence on the consumption of leisure—and hence reduce work—but that raising a person's wage rate will (other things equal) induce a person to substitute work for leisure. That is, higher income tends to *reduce* work (the "income" effect); higher wage rates per se tend to increase work (the "substitution" effect).

Of course, there is always the nagging possibility that economists have learned their theory too well, have a prior belief in those qualitative characteristics of labor suppy, and continue to permute samples, variables and functional forms until they obtain results they can be comfortable with.[8] This, of course, does not destroy the possibility that prior hypotheses can be refuted by data. But it should be kept in mind as a qualification against interpreting this conformity as yet another independent confirmation of standard theory.

By other criteria for conformity with a priori notions, however, the collection of estimates offer mixed results. As noted above, the two dominant changes in the labor force over time have been a longrun decline in the labor force participation by males and a longrun increase in market work by females. Among the male groups studied the total income elasticity (tending to reduce work) is usually larger in absolute value than the substitution elasticity (tending to increase work)—a finding which is consistent with the empirically verified longrun decline in the labor supply of males—although there are a number of exceptions to this result. Among the majority of the studies the substitution elasticity is larger in absolute value for females than for males, which is also consistent with relevant time-series data.

For those who weigh heavily the question of the work disincentive features of income maintenance legislation, however, general qualitative agreement such as shown in tables 1 and 2 is not enough. Quantitative magnitudes are critical, and it makes a major difference whether the overall net reduction in labor supply on the part of the working poor is, say, 4 percent or 40 percent. Estimated responses implicit in these studies span a range at least this wide. Unfortunately, such divergent estimates are of little use to the policymaker. He cannot judge the potential costs or benefits of an income transfer program without more consistent estimates of their impact on work.

The largest elasticity of substitution for prime age males that has so far been published is from the study by Kalachek and Raines, published in the *Technical Studies* of the report of the President's Commission on Income Maintenance. This estimate, around 0.9, exceeds by a wide margin the substitution elasticities found in the seven newly published studies listed above, the largest of which is 0.5

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[8] Greenberg and Kosters obtained the conventional negative income effect only after devising an "asset preference" variable. (This procedure is discussed later.) To cite another example, the negative income effect which Hill uses in his computation of elasticities is based on a nonlabor income variable consisting of transfer payments such as public assistance, unemployment compensation, and pensions. Another of his income coefficients, using nonlabor income from fully annuitized family wealth, was positive (although insignificantly different from zero).

(reported by Hill).[9] Ignoring the few cases of negative substitution elasticities, we see that the smallest elasticities are close to zero. An even larger range of variation in the estimates of substitution parameters for females is shown in table 2. The estimates of income parameters are only slightly less variable.

In comparison with the reported range for price and income elasticities of food, clothing, rent, and so forth, the degree of disparity among these estimates does not seem excessive.[10] However, the estimates in tables 1 and 2 are all (except for the study of Fleisher et al.) based on *similar* data (often the *same survey*), similar time periods, and very similar populations. This makes the disparity more conspicuous and more disconcerting.

The effect of an income maintenance plan on the supply of labor cannot be estimated on the basis of the income and substitution parameters alone. The estimates also depend on the way the simulation of the plan is applied, the definition of the population covered, and the values of the variables assumed. Below we show several of the authors' own simulations of similar plans, which reveal very clearly a large range of estimates of labor-supply reduction.

> Kalachek and Raines predict that an income maintenance plan providing a $2,400 annual guarantee (for a family of four) and a 50-percent tax (benefit-loss rate) on earnings would produce a 46-percent reduction in the labor supply of the eligible population. (Male family members would reduce their labor supply by 37 percent.)[11]
>
> Greenberg and Kosters (1) predict that a $2,400 guarantee and a 50-percent tax would cause a 15-percent reduction in the labor supply of male heads of covered families.
>
> Garfinkel (5) predicts that a $3,000 guarantee and a tax rate of 50 percent would reduce the labor supply of prime age, able-bodied husbands under the plan by anywhere from zero to 3 percent.

How can this range be narrowed to provide some guidance for policy? Clearly, the natural experiment which the labor market has performed to generate the observations for the user of survey data—assuming that the experiment of conducting household interviews faithfully records the operation of the labor market—is not the same as that which would occur if a negative income tax plan became law. And certain difficulties are inherent in any attempt to use such survey data for making inferences about the sorts of income and substitution effects applicable to recipients of an income maintenance plan like FAP.

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[9] Green and Tella report in an early study estimates of substitution elasticities that are also relatively large, averaging about 0.5 in 1965 and 0.8 in 1966. However, Rosen and Welch, "A Note on the Estimation of Labor Supply," *Journal of Human Resources* (winter, 1972), have pointed out some flaws in this study. We have chosen to include in tables 1–2 the later study (by Tella, Tella, and Green) which appears to be methodologically superior. See also R. F. Hoffman and B. R. Schiller, "Work Incentives of the Poor: A Reconsideration," *Review of Economics and Statistics*, v. LII, November 1970, pp. 447–449, and the "Reply" by Green and Tella in the same issue for a further discussion of the difficulties in interpreting the Green-Tella article.

[10] See, for example, A. S. Goldberger and T. Gamaletsos, "A Cross-Country Comparison of Consumer Expenditure Patterns," European Economic Review, vol. 1, spring 1970; and H. S. Houthakker, "New Evidence on Demand Elasticities," *Econometrica*, v. 33, April 1965, pp. 277–88. The authors in both papers incidentally, remark on the wide range in price and income elasticities.

[11] These estimates are reported in Edward Kalachek and Fredric Q. Raines, "Labor Supply and the Negative Income Tax," an unpublished paper.

The problems of making inferences about behavior under a specified set of conditions from behavior observed under different conditions may be discussed in terms of several questions:

What sample observations should be included? What measure of labor-supply behavior should be used as the dependent variable? What selection of wage and income variables, and what other control variables, should be included as independent variables? These are probably the most critical, although others can certainly be mentioned, such as the choice of functional form to relate the dependent and independent variables, or the choice of an estimation technique. The studies in this volume cope with these questions in various ways, and partly for this reason they reach widely divergent predictions. The next three sections discuss these inherent difficulties and the techniques used by the different authors to get around them.

III. SELECTION OF THE SAMPLE TO BE ANALYZED

An income maintenance program will make major changes in income and effective wage rates only for the lower part of the income distribution. At first glance, therefore, it might appear reasonable to restrict the estimation model to the low-income families likely to be affected. If one believes that income and substitution parameters for the poor will be different from those for the nonpoor—that there are interaction effects—it might be the indicated procedure. The existence of such an interaction effect is consistent with the belief that the poor are less disposed to work, and with the fact that the poor generally have less pleasant jobs than the nonpoor.

Two points deserve to be made here. First, the serious attempts to characterize a "culture poor" group with markedly different and stable motivational patterns have produced rather small numbers of so-afflicted persons relative to the total "income poor" or to the number eligible for prospective income maintenance policies. Second, the current labor force activity of this ill-defined group is already tenuous or nonexistent, making any examination of the possible work disincentives for them more or less academic. A plausible, though speculative argument could be made that a more stable base of income could give substantial help toward self-support to such highly disorganized, alienated, and variously impaired persons.

But one does not have to postulate a culturally distinct group of "poor" in order to justify concentrating analysis on a somehow defined sample of poor or low-income workers. Work behavior might be quite continuous through wide ranges of earning ability and unearned income, but nonlinear in an unknown way. Here an analyst could choose a strategy of finding a linear approximation in the neighborhood (for example, low wage, low income) where he intends to draw conclusions or make projections.

There is a fundamental difficulty, however, with such an approach that estimates income and substitution effects separately for the poor and the nonpoor. Survey data do not permit an identification of the "normal" or "permanently" poor as distinct from the "normal" nonpoor who may be having a bad year. In other words, the "normal" or "permanent" wage rate and amount of income from nonemployment sources is not ascertained. The use of current income, truncated to eliminate from the regression equation all groups above, say, the poverty line, produces (in consequence) biases in the resulting estimates of income and wage effects on labor supply. How far above the poverty line should the cutoff be made to get around this problem? Or should there be no cutoff at all? Ashenfelter-Heckman, Boskin, Cohen, et al. and Garfinkel do not use an income cutoff in selecting observations. Hall excludes observations mainly on the basis of a predicted wage which is a function of putative exogenous variables. All the other studies reported in tables 1 and 2 use a measure of current income, primarily labor income, and thus truncate the sample; this is a common problem that is worth discussing in some detail.

The problem of truncating samples[12] can be explained in its most basic form first in a heuristic manner by noting that if two populations have essentially different behaviors which one desires to estimate separately, then one should seek two samples which unambiguously represent the two separate populations. If the two samples are formed by an inaccurate discrimination between the two populations, this representation will be violated, and biases will be produced. Clearly, in the present case one would not expect that measured income (with all its transitory fluctuations) in a particular period would provide an accurate basis for discriminating poor from nonpoor.

Now consider a highly simplified case with more rigor. Consider a model which specifies an observed income quantity as the sum of a general function of a set of variables X and an additive random disturbance:

$$y_i = g(X_i) + u_i. \tag{1}$$

The function, g, which we shall take as unknown, provides the conditional expectation of y given the vector X. Now suppose we want to estimate the function, g, in that portion of its domain where its value is less than some value—say the poverty level, y_p. Suppose, moreover, that we attempt to do this by limiting our sample to those observations which have an observed value, $y_i < y_p$. Now consider the probability that an observation will fall in the sample we have chosen. This requirement implies, using (1), that:

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[12] See the Rosen and Welsh discussion (cited in footnote 9) of the Green and Tella truncation bias. Our analysis has also benefited from our discussions of this problem with Richard Toikka.

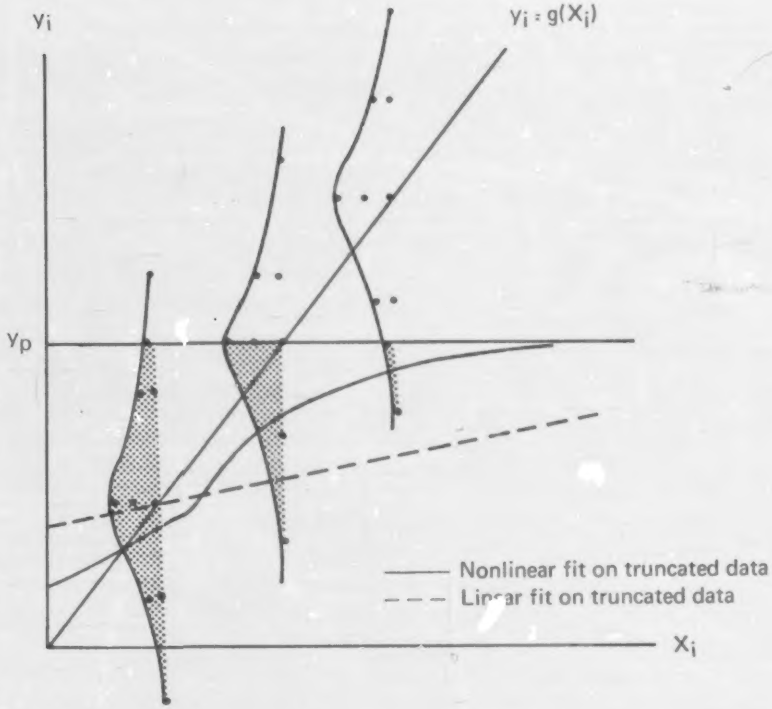
$$y_i < y_p$$

$$u_i < y_p - g(X_i).$$

Obviously, if the conditional expectation of y is well below y_p relative to the standard deviation of u, the probability that u is smaller than this difference will be close to 1. But as one considers cases where the conditional expectation of y approaches y_p, the probability is close to 0.5 that this observation will be included. Moreover, if one considers cases where the expectation $g(X_i)$ is outside the domain of interest, there will still remain a finite probability that u will be negative enough to throw the observation into the specified sample. Indeed this probability also gets close to 0.5 when the expected value is just above y_p.

If one considers next the expected value of y for the cases that are included in the sample, it is clear that these will lie below $g(X_i)$ for the cases where $g(X) < y_p$. This divergence gets larger as $g(X_i)$ approaches y_p from below and is equal to the mean of the lower half of the (symmetrical) distribution of the disturbances. The sample will also be adulterated by the wrong population, that is, those which satisfy $g(X) > y_p$. The effect of these depends mostly on the fact that they were included in the sample because they have unusually large negative disturbances, and moreover that the X vector associated with them lies outside the subspaces which produce $g(X_i) < y_p$. These observations then will act in much the same way to distort the estimates obtained from the restricted sample away from $g(X_i)$ in a negative direction. The effect of all this on individual coefficients depends upon how a particular equation is specified for estimating $g(X_i)$. If the specification is of a simple linear nature, then all the coefficients will be biased toward zero. If enough flexibility is provided for curvilinear relationships, the estimated function approaches y_p from below. Figure 1 indicates the nature of the problem in the simple one-variable case.

FIGURE 1. EFFECT OF A TRUNCATION OF Y ON THE FITTED REGRESSION RELATION OF Y ON X


y_i
$y_i = g(X_i)$
y_p
X_i
Nonlinear fit on truncated data
Linear fit on truncated data

Note: Each dot represents some fixed number of observations, (X_i, y_i). A normal distribution of values of y for a fixed value of X is assumed. The shaded areas designate the part of the distribution that is included in the truncated sample.

For the case at hand the situation is somewhat more complicated because the regressions use hours as the dependent variable and the sample is truncated by eliminating families with high income. Letting H=hours; w=wage rate; NEY=nonemployment income; y_m=income cutoff level; and Z=a vector of control variables. Here, then, only families that satisfy

$$y_i=w_iH_i+NEY_i<y_m$$

are included in the sample. But since the hours equation is written,

$$H_i=g(w_i,\ NEY_i,\ Z_i)+u_i,$$

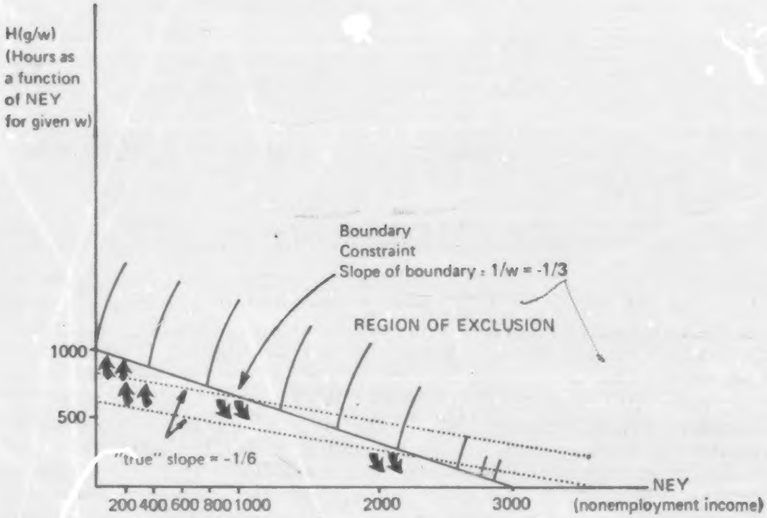
we have again the situation that the distribution of u will be truncated for some values of the arguments of g. Specifically, only the u_i which satisfy the expression,

$$u_i<\frac{y_m-NEY_i}{w_i}-g(w_i,\ NEY_i,\ Z_i),$$

are admitted. Since the first term on the right of the inequality involves both wages and NEY (non-earned income), the effect of truncation cannot be determined without further information. The information required is the slope of the function g—we assume a simple linear function—with respect to, say, NEY. We know that the slope of the boundary expression is $-1/wi$. If the slope of g is less steep but still negative it is clear that for higher values of NEY (lower values of g) the upper part of the distribution of u will be eliminated (see figure 2). This will cause the slope estimated from the truncated sample to overstate the negative effect of NEY. Similar reasoning can obtain the whole set of conditional biases.

FIGURE 2. EFFECT OF AN INCOME MAXIMUM TRUNCATION ON THE SLOPE OF THE INCOME-HOURS RELATION (HOLDING THE WAGE FIXED)

Assume: Y max = \$3000; w = \$3.00/hour; $\partial H/\partial Y = -1/6$
w = wage rate
$\partial H/\partial Y$ = change in hours with respect to income


H(g/w)
(Hours as
a function
of NEY
for given w)
1000
500
Boundary
Constraint
Slope of boundary : 1/w = -1/3
REGION OF EXCLUSION
"true" slope = -1/6
200 400 600 800 1000
2000
3000
NEY
(nonemployment income)

↑↑ Note: Direction of bias or "tilt" in fitted line in the presence of the ↓↓ boundary constraint. The curve is tilted towards the -1/3 slope and the fitted curve is less than -1/6 in slope.

In the particular case of the *NEY* slope, it can be shown that

$$\frac{dg}{dNEY} > -\frac{1}{w},$$

provided that cash income (or "wage goods") are normal goods. With the assumption, then, that not all of an increase in *NEY* will be consumed in the form of increased leisure or nonmarket activities, we can say that truncation on income will cause an overstatement of the negative income effect.

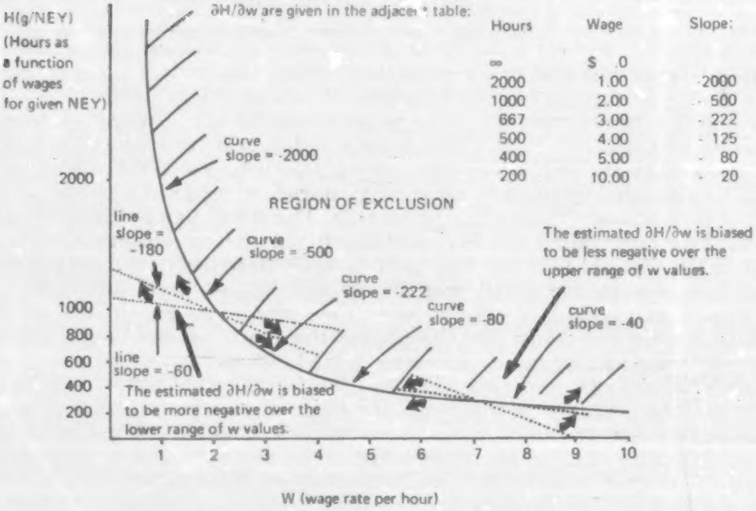
An illustration is provided in figure 2, where the y_m (income cutoff) value is arbitarily chosen to be \$3,000, and the given wage is chosen to be \$3 per hour. If the true slope is $-1/6$, which satisfied the condition that $dg/dNEY > -1/w$ $(= -1/3)$, then, as *NEY* increases, u_i must take on increasingly large negative values to permit the inclusion of observations in the sample. The fitted line is tilted downward for high values of *NEY*, and any fitted linear relation expressing $\partial g/\partial NEY$ would become steeper in slope or more negative.

The bias in the fitted wage/hours relation is more complicated, because the boundary expression of the wage/hours graph is a negatively sloped rectangular hyperbola, the slope of which varies at every point. In figure 3, the same $y_m = \$3,000$ and an assumed value of $NEY = \$1,000$ requires that the boundary satisfy the condition that $H \times w = \$2,000$, so the slope, dH/dW, will equal $-(2000/W^2)$. For relatively low values of w, say \$2 or \$3, the boundary slope is steep, -500 and -222 respectively. These values exceed the measured slopes of dg/dw, which are around -60 (for Ashenfelter and Heckman) and -180 (for Fleisher, et al.). A truncated sample *over this range of values of w* would tilt the fitted relation to be steeper or more negative. On the other hand, at higher values of w, the slope of the boundary is flatter and will exceed the true slope of dg/dw. At $g = \$7.00$, the fitted slope is made less steep and less negative.

FIGURE 3. EFFECT OF AN INCOME MAXIMUM TRUNCATION ON THE SLOPE OF THE WAGE-HOURS RELATION (HOLDING NONEMPLOYMENT INCOME FIXED)

Assume: Y max = \$3000; NEY = \$1000

Given the boundary condition H × w = 2000, the boundary slope is $\partial H/\partial w = 2000/w^2$. For various values of w, values of H and $\partial H/\partial w$ are given in the adjacent table:



Hours	Wage	Slope:
∞	\$ .0	
2000	1.00	-2000
1000	2.00	- 500
667	3.00	- 222
500	4.00	- 125
400	5.00	- 80
200	10.00	- 20

Note: Direction of bias or "tilt" in the fitted line in the presence of the boundary constraint. Boundary slopes are drawn to a scale in which 1 *w* unit = 400 *H* units and all slopes are correspondingly 1/400 of their stated values.

Given the range of values of *w* and *NEY* for the families in the truncated samples, it is likely that both the negative income effect and the uncompensated (usually negative) wage effect are biased in a negative direction. If, as appears likely, the income effect has the more severe bias, then the computed substitution effect

$$\left(= \frac{\partial ws}{\partial w} = \frac{\partial H}{\partial w} - \frac{\partial H}{\partial w} - \frac{\partial H}{\partial Y}\overline{H}\right)$$

would be biased up. A larger positive substitution effect and a large negative income effect would, of course, result in a larger reduction in labor supply for any income maintenance plan.

Despite the pitfalls encountered in fitting labor supply functions to the low-income group alone, however, the possibility that an interaction specification is correct remains to nag those who decide against stratification. Simply using dummy variables to denote low-to-high wage variables (as several authors did) may be useful, but this procedure does not really capture the intended interaction—which requires that the wage variables interact with income variables (or with other variables representing income status). It is, however, possible to specify or test these interactions explicitly in an expanded model.

Another plausible and econometrically defensible basis for deciding to restrict the sample—as a means of improving the estimates of wage and income parameters for purposes of measuring how the working poor would be affected by a program like FAP—is to eliminate the totally (or partially) disabled, the aged, those on the welfare rolls, and other such categories of nonworkers. The reasoning behind this approach is that a very different model probably relates observed wages and labor supply for such households. One way of looking at this difference is to consider that, instead of wages "causing" work decisions, a set of other exogenous variables—like ill health or being old—are causal both to their observed wages and to their labor supply. An alternative way of putting this is to say that their "tastes" or "preferences" for work are substantially different from those that characterize the working poor. In either case the wage/labor supply relation would provide a spurious estimate of the postulated dependent relation of labor supply on wages which holds for the working poor.

This topic will be discussed further in the section about the choice of independent variables. Suffice it to say here that the inclusion or exclusion of such groups is probably a major source of the varied estimates of work reduction made in these studies.

IV. MEASURING LABOR SUPPLY: THE CHOICE OF THE DEPENDENT VARIABLE

Since concern about the disincentive effects of an income maintenance plan usually centers on the reduction in hours worked and the subsequent drop in earnings (and GNP), it would seem natural to use a measure of time spent working as the dependent variable in regression models of labor supply. Most of the papers specified have measured labor supply in this way, but the issue is not beyond dispute. Several authors—Hill (4), Garfinkel (5), and sometimes Greenberg and Kosters (1)—have measured the labor supply in terms of the conventional textbook definition of an "offer" function, which combines time spent at work with time spent looking for work. Operationally, this amounts to adding time unemployed to time employed. The sum defines labor force participation—a common measure of labor supply in the literature.

Which of these two measures of labor supply is correct depends on the question asked. A focus on current market work and money income calls for the "time employed" concept. However, if unemployment is viewed as job search activity that is intended to increase output in the future at the expense of current output, we are led to a more expansive measure of total time in the labor force. One difficulty with the latter focus is that, for consistent application, it would require that time spent in schooling also be combined with time in the labor force, since schooling also is an investment in increased productive capacity.

The choice between measuring employment versus measuring labor force participation involves another issue that has received some attention in the literature—namely, the implicit constraints on adjusting one's labor supply time over the period covered by the survey interview. For adult males in particular, the employment decisions are to some extent restricted to working full time—that is, roughly 40 hours a week the year round—or to not working at all. However, over the course of a year it is likely that some flexibility is likely to be achieved by means of time between jobs—in absenteeism, or time on layoffs, or other forms of unemployment. Perhaps one manifestation of a positive substitution effect between "labor supply" and the (potential) wage rate is in an inverse relation between these modes of not working and the wage rate. All such modes are likely to be reported as "unemployment" by adult males in answer to survey questions. By this interpretation there is even less flexibility in the labor supply of adult males when both employment and unemployment time are combined—that is, the wage or income responsiveness of labor supply for adult males is probably less than when employment time alone is the measure used.

Of course the nature of any analysis which separates labor force participants for conditional analysis of quantity of labor supplied depends importantly on the time interval over which participation is observed. Longitudinal data, such as used by Fleisher et al., have important advantages over the traditional data for monthly labor surveys, which use only 1 week's experience. The latter kind of data undoubtedly turn up many more nonparticipants and not-currently-employed persons than data covering an entire year. As mentioned above, a substantial part of the ability of a worker to adjust his supply may well come from ability to adjust the length of intervals working, looking for work, et cetera. In a large cross-sectional snapshot, reliable averages of these various statuses can be obtained, but the argument that those who happened to work during the survey period are behaviorally very different from those who didn't is less persuasive when one examines the past week rather than an entire year.

Even if the labor supply measure is restricted to some measure of time spent at work, there remains a variety of work measures to choose from as shown in table 3. Undoubtedly, the most important question is whether and how to include those who were not in the labor force. There are three principal ways of dealing with the nonparticipants. The method used by Boskin (3) (and by Kalachek and Raines) consists of separating the work decision into two stages: the first being the choice of whether to seek work or not; the second being the choice of how many hours to work. The "full" labor supply concept is, therefore, determined by the product of these two separate functions. A second method is to include the nonparticipators as ordinary zero values in the single equation for hours of work as the measure of labor supply. Hall adopts this procedure. The third method, used by Garfinkel, Hill, Fleisher et al., and Greenberg-Kosters, just excludes the nonparticipators from the regression.

TABLE 3.—*Selected alternative measures of labor supply* [1]

Measure	User	Data source [2]
1. Weeks worked last year	See measures 4, 8, and 9.	SEO, CPS, NLS, Censuses.
2. Weeks worked last year plus weeks unemployed last year.	Garfinkel	SEO.
3. Hours worked last week	See measures 4, 5, and 14.	SEO, CPS.
4. 3 × 1	Tella, Tella, and Green.[3]	SEO.
	Parker [4]	SEO.
	Rosen and Welch.[5]	.001 Sample of 1960 Census.
5. Average weekly hours worked last year × 1.	Fleisher, Parsons, and Porter.	NLS.
6. 3 × 2 (defined for nonzero values of both 3 and 2).	Greenberg and Kosters.[6]	SEO.
	Cohen, Rea, and Lerman.	CPS.
7. Estimated weekly hours of work during last year: 40 if person was primarily a full-time worker last year; 30 if primarily a part-time worker.	See measures 8 and 9.	SEO.
8. 7 × 1	Hill	SEO.
9. 7 × 2	Hill	SEO.
10. Dummy variable (for individuals): 1 if in the labor force last week; 0 otherwise.	Bowen and Finegan.[7]	.001 Sample of 1960 Census.
Labor-force-participation rate (LFPR) (for groups), based on labor-force status last week.	Ashenfelter and Heckman.	1960 Census.
11. Dummy variable: 1 if worked last year; 0 otherwise.	Kalachek and Raines.	CPS.
	Boskin	SEO.
12. Dummy variable: 1 if primarily a full-time worker last year; 0 if primarily a part-time worker.	Garfinkel [8]	SEO.
13. Earnings last year/wage rate last week (where wage rate last week = earnings last week/hours worked last week).	Not used, but see measure 14.	SEO.
14. Earnings last year/predicted wage rate earned last week (where predicted wage comes from a regression using reported last week's earnings).	Hall	SEO.
	Boskin (see measure 16).	SEO.

Footnotes at end of table.

TABLE 3.—*Selected alternative measures of labor supply* [1]—Continued

Measure	User	Data source [2]
15. "Probability" of 11 × (3 × 1) where the probability is obtained from a regression using 11 and (3 × 1) is confined to those with nonzero values of 3 and 1.	Kalachek and Raines.	CPS.
16. "Probability" of 11 × 14 (see description under 15).	Boskin	SEO.
17. Earnings (or earnings change) with data at two points in time.	Fleisher, Parsons, and Porter.	NLS.
18. Years worked/years married (for wives) (a labor-supply concept that approaches a measure of a lifetime quantity of labor supplied).	Morgan et al.[9] Cain [10]	SRC Survey GAF.

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[1] The list is by no means exhaustive but does provide a description of the labor-supply studies shown in tables 1 and 2, as well as some interesting alternative measures used in other studies. The definition of labor supply must be considered as "approximate" since each author carried out various refinements and special procedures with the data.

[2] SEO=Survey of Economic Opportunity. CPS=Current Population Survey. Census=Decennial census, including the .001 (or 1/1000) sample of the 1960 Census. NLS=National Longitudinal Survey (here, for men age 45 to 59, in 1966–67). SRC=Survey Research Center at the University of Michigan, survey in 1959 and reported in James Morgan et al., *Income and Wealth in the United States* (New York: McGraw-Hill & Co., 1962). GAF=Growth of American Families fertility survey in 1955 and reported in Glen G. Cain, *Married Women in the Labor Force* (Chicago: University of Chicago Press, 1966).

[3] Mean values defined for nonunemployed groups of wage-income classes. (Nonunemployed groups refer to persons who are either employed or not in the labor force.)

[4] Defined for 4 types of workers: full-time, full-year; full-time, part-year; part-time, full-year; part-time, part-year.

[5] Defined for nonzero values of both 3 and 1.

[6] David H. Greenberg and Marvin Kosters report that they experimented with a labor-supply measure defined as 3 × 1 and as 2 × estimated hours last week (24 if the person worked part time voluntarily; 40 if he worked part time involuntarily; actual hours if he worked 40 or more).

[7] This measure has been used with individual data by many, but most extensively by William G. Bowen and T. Aldrich Finegan. Aggregative data for standard statistical metropolitan areas (SMSA's) were used to define LFPRs for the group studied. Another variation of this measure, used by Cain, for nonwhite wives with the 1960 Census data for SMSAs, was LFPR × weeks worked by wives who worked, which thereby weights the participation rate by the amount of work.

[8] Garfinkel also experimented with the dummy variable: 1 if primarily worked full time last year or involuntarily worked part time; 0 if worked part time voluntarily last year.

[9] See James Morgan et al., *Income and Wealth in the United States*.

[10] See Glen G. Cain, *Married Women in the Labor Force*.

The argument for including the nonparticipators (other than simply to increase the sample size) is that the decision to stay out of the labor force is an important way in which persons can affect their labor supply in the short run of a year or so. (We note again our view that once the decision is made, flexibility in hours worked is greatly diminished, especially for adult males.) The argument for separating the labor supply function into two separate stages is that the way in which the independent variables influence labor supply is different at each stage; that there is a discontinuity in the labor supply function at the zero/nonzero point on the hours dimension.

The arguments for excluding the nonparticipators is that they bring into the regression "deviant" sample points–deviant in that the nonparticipators (especially when considering prime-age male heads of households) are: (*a*) at a point of disequilibrium and not representative of a normal labor supply function; (*b*) markedly different from the "working poor" because they have various unmeasured mental and/or physical maladies that keep them from working; (*c*) markedly different in their tastes for market work versus alternative uses of their time. If these propositions are correct—as implied by Fleisher et al. (7), Garfinkel (5), and Greenberg-Kosters (1)—then there may be more lost because of the resulting distorted wage and income effects than is gained by capturing the "no work" dimension of labor supply. As Garfinkel illustrates, it does appear that including nonparticipators tends to increase the calculated wage and income effects on labor supply.[13] This is, of course, consistent with either of two hypotheses: (i) the nonparticipants introduce a correlation between tastes "against" work and wages and between such tastes and the receipt of nonlabor income on the part of the poor (as is discussed in the next section)[14]; or (ii) the "longrun" equilibrium wage and income response (which is assumed to be larger than shortrun response) is better measured when the nonparticipation choice is included.

One of the strengths of the Survey of Economic Opportunity as a cross-section body of data is that it provides an unusual amount of detail and a variety of measures of labor-force activity (see table 3). By and large, the studies using these data have so far not taken advantage of this potential (which, as usual, sounds easier to do than it is). The papers discussed here, for instance, have used one measure at a time from this array, instead of combining them in order to average out partially offsetting errors. Alternatively, the various measures could be used for checking consistency.

V. THE SPECIFICATION AND MEASUREMENT OF INDEPENDENT VARIABLES

Although formal mathematical statistics and the appropriate assumptions underlying the application of least squares may be used to evaluate the question of bias of the independent variables of interest—mainly wage and income variables—it may be fruitful to approach the subject in a more informal, descriptive fashion. The problems that arise in the measurement and functional form of wage and income variables in the model may be grouped into three categories familiar as problem areas in econometric texts: (*a*) simultaneity; (*b*) omitted variables; and (*c*) errors in the variable.

In the discussion that follows we are not seeking a "true" model of labor supply, with "true" measures of wage and income variables and parameters. The term, "true" has no meaning except in the context of a specific process (or "experiment") conducted with a specific population. In the problem under study here, the process consists of changing (reducing) effective wage rates and providing income transfer payments to the "working poor" who are not now receiving welfare assistance. The challenge is to use the pseudo-experiment of market-produced, historical changes in wages and income as reported in survey data to simulate the actual reality of an income maintenance program.

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[13] This point is also brought out in Parker's findings, which, as shown in tables 1-2, show *negative* substitution effects and, usually, zero income elasticities when his hours-worked regressions are confined to full-time/full-year workers. His estimates are more positive for substitution effects and are more negative for income effects when he uses observations of workers who worked less than full-time/full-year. Fleisher et al. have also supplied us with regression results (reported in table 1) that illustrate this sensitivity.

[14] Several unpublished papers have examined the implications for labor supply studies of a correlation between "tastes for work" and labor force participation. Yoram Ben-Porath, "Labor Force Participation Rates and the Supply of Labor," Discussion Paper No. 206, Harvard Institute of Economic Research, Harvard University, Cambridge, Mass., September 1971; Reuben Gronau, "The Intra-family Allocation of Time: The Value of the Housewife's Time," Research Report No. 28, Department of Economics, the Hebrew University of Jerusalem, June 1971; H. G. Lewis, "Labor Force Participation Rates and the Theory of Hours of Work," Department of Economics, University of Chicago, 1967.

A. The Problem of Simultaneity

A primary source of potential simultaneity lies in the fact that interrelations of work decisions among family members can affect the labor supply function in various ways.

(1) If cross-substitution price effects are present, then the wage rates of each family member belong in every member's supply function. Most of the authors of labor supply studies have, however, assumed a zero cross-substitution effect and treat the earnings of other family members as producing only income effects.

(2) Another plausible interrelation can be specified by including the quantities of time worked by each family member in the labor supply function of each of the other family members. The expected sign of the other member's wage rate, then, depends on whether the work/nonwork activities are complementary or substitutable as between (or among) family members. But since no one claims that an individual's wage rate (plus other available variables) fully explains the quantity of time he or she spends at work, it follows that some variation in the quantities are determined by additional variables external to the model used. Empirically, the unexplained variation in quantities of labor supplied is substantial, and it would seem reasonable to enter all other members' quantities of work explicitly on the right-hand side of the labor supply equation. This specification clearly requires simultaneous-equation models for estimation of the wage and income parameters.

(3) Interrelations among the labor supplies of family members are also a part of the more general problem of estimating nonmarginal changes in the labor supply quantities. (The Ashenfelter-Heckman paper (6) provides a rigorous treatment of this issue.) The changes in income from changing the wage rates of, say, the husband and wife is measured by $dW_h\overline{Q}_h$ and $dW_w\overline{Q}_w$, where the $\overline{Q}_h$ and $\overline{Q}_w$ are assumed to represent equilibrium levels of labor supply of husband and wife, respectively.[15] Now, the induced changes in quantities change the equilibrium values of $\overline{Q}_h$ and $\overline{Q}_w$. The ability to measure the substitution effect of a wage change by holding income constant is, therefore, legitimate only for infinitesimal changes around the "old" equilibrium levels. Clearly the changes in Q_i may not be "marginal" following the institution of a negative income tax plan, and this sets up the likelihood of feedback effects from right-hand to left-hand side of the equations.

Another source of potential simultaneity in the labor supply equations reported in tables 1 and 2 involves the wage variable. There are several sources of potential trouble. First, it is likely that the amount

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[15] Let total income, $Y_t = Q_hW_h + Q_wW_w + Y_n$ where Q_h and Q_w are hours of work of the husband and wife, respectively; W_h and W_w their wage rates; and Y_n is nonlabor income. An income-maintenance program will change Y_n, W_h, and W_w for the eligible population, and the effects on Q_h and Q_w may be expressed as follows (assuming no cross-substitution effects):

$$dQ_h = S_h dW_h + B_h(\overline{Q}_h dW_h + \overline{Q}_w dW_w + dY_n)$$
$$dQ_w = S_w dW_w + B_w(\overline{Q}_w dW_w + \overline{Q}_h dW_h + dY_n)$$

where S_i is the own-substitution effect and B_i is the income effect.

of time offered by a worker will partly determine the wage he receives. Full-time workers, in particular, may be able to command a higher wage than part-time workers.

Second, the fact that the wage variable is based on the individual worker's earnings means that it is a different animal from a wage rate that confronts him in the market, which is exogenous and over which he has no control. Within a geographic labor market it might be expected that most of the wage variation is attributed to equalizing productivity and/or compensating nonpecuniary differences—that there is only one price (adjusted for nonpecuniary differences) for a given unit of labor productivity. To assume that labor supply variation associated with the wage variation represents a causal relation is to assume first that the productivity differences among workers are by themselves unrelated to supply differences. (This point is taken up further below, in discussion of omitted variables.) However, the productivity differences are partly endogenous, particularly in the longer run, since they will depend on the decisions the individual makes about investments in human capital, residence, tradeoffs with nonpecuniary considerations, and other choices. Second, even at a moment in time in a survey, the wage received by the worker may depend partly upon the worker's own choice among a variety of employment-compensation packages, in which the money wage is only one component.

The question that arises, then, is whether the process by which the sample observations are generated is one in which a common set of variables jointly determine *both* the quantity of labor supplied and the wage, and whether disturbances in the two variables are thereby correlated. As in so many questions raised in this chapter, there appears to be no certain answer.

A third problem that has arisen in every attempt with survey data to regress hours of work on a wage rate measure as a regressor is that the two variables are definitionally related. A wage rate is defined as some measure of earnings divided by a measure of hours, and the analyst must make do with a dependent variable which appears as a component of the measure of a critical independent variable. Given some errors of measurement in hours and wages, some correlation of the disturbance term and the independent variable is nearly assured. This source of bias is most fully discussed by Hall (2).

B. Omitted Variables and Bias in Included Variables

The first issue to be raised here is the potential bias in the measure of wage and income effects caused by omitted variables that are correlated both with these and with labor supply.[16] The most likely candidates are: (*a*) preferences for work relative to nonwork activities;

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[16] Given a relation of interest, $y = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + e$, the alternative relation which omits X_2, $y = \alpha_0 + \alpha_1 X_1 + u$, is said to provide an estimate, α_1, that is biased with respect to the relation between y and X_1 that is represented by the first equation. The nature of the "bias" is shown by the following expression for β_1,

$$\beta_1 = \alpha_1 - \beta_2 b_1,$$

where b_1 is obtained from the "auxiliary regression" between the "omitted variable," X_2, and X_1—that is,

$$X_2 = b_0 + b \cdot X_1 + e.$$

Thus, α_1 is a "biased" measure of β_1 whenever β_2 and $b_1 \neq 0$.

(*b*) skills and/or productivity in relevant nonmarket work activities like home production; and (*c*) various unmeasured traits affecting wage, income, and labor supply such as the quality of education, training, work experience, and mental and physical health.

The general point about preferences is that personality traits—ambition, the protestant ethic, a desire to retire in comfort or to leave abundant material goods to one's heirs, a dislike for spending time at home, or any number of other characteristics—could be causal to decisions to obtain high wages or to accumulate nonhuman wealth and to work a lot in the market. Clearly, since an income maintenance program will change the effective wage rates and nonlabor income across all families in the eligible population, the information we are looking for is the partial relationships between wage rates and income on labor supply, holding personality traits constant. Since the variables available in survey data offer at best meager control over such traits, the resulting estimates of wage and income effects on work effort may well be biased.

If the traits were like to those listed in (*c*) above, the measured wage effects would be more positive than the partial effects we are interested in. By the same reasoning, the income effect is likely also to be biased in an upward (positive) direction. This positive bias in the income effect is likely to be stronger the more the observations include families in the middle and upper income ranges. (Among poor families, the receipt of nonlabor income is much more likely to be associated with work-conditioned sources, such as welfare payments, unemployment compensation, pension benefits, and the like, all of which produce a large negative relation with the quantity of labor supplied, for reasons discussed earlier.) Indeed, Greenberg and Kosters, who used a relatively high income cutoff of $15,000, did estimate a positive income effect. Their rationalization was the positive correlation with an unobservable "preference for asset accumulation," and they were led to create and include in their labor-supply model a proxy variable for this type of preference. The created variable, *P*, was defined as:

$$P=\frac{\text{actual (observed) net worth}-\text{predicted net worth,}}{\text{predicted net worth}+\text{human capital}}$$

where predicted net worth is determined by regressing observed net worth on age, the wage rate, and the wage rate squared. Since these variables—net worth, age, and the wage rate (and sometimes the wage rate squared)—were also included in their regression model estimating labor supply, along with the created variable for preferences, the interpretation of the derivative of labor supply with respect to net worth (or nonlabor income) as well as with respect to wages and age is somewhat ambiguous, even though their signs become "theoretically correct." Greenberg and Kosters claim that the explicit coefficients on net worth (or nonlabor income) and on the wage are measures of the income and substitution effects respectively, while the implicit coefficients on net worth (or nonlabor income) and on the wage *as components of the created preferences variable* are not income or substitution effects but are, in fact, preference effects.[17] Presumably, analysts may argue with this interpretation, but the example serves to illustrate the difficulty of estimating relations under conditions in which the variables subject to direct policy change are correlated with unobservable determinants of the behavior being studied.[18]

It is interesting to contrast the interpretation of the preferences variable by Greenberg and Kosters (1) with the interpretation of a similarly defined variable—in this case, the actual (observed) dollar value of assets minus the predicted value of assets—by Fleisher et al. (7). The latter view the difference as representing a deviation of actual assets from desired assets, and postulate that an excess of actual assets over desired (predicted) assets indicates that the household has "too many" assets. Therefore, they expect the excess to be negatively related to market work—less work being a way of restoring an equality between actual and desired assets. Greenberg and Kosters, on the other hand, postulate that an excess of actual assets over predicted assets indicates a preference for work (that is, preference for asset accumulation) and, therefore, is expected to be positively related to work.

How can these two hypotheses be reconciled? One way is to view the Greenberg-Kosters formulation as holding when the households are in equilibrium (with respect to labor supply and asset accumulation), whereas the Fleisher et al. formulation characterizes households in disequilibrium. (This raises the question of which assumption about equilibrium status is the more reasonable when using a particular data file.)

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[17] Let the wage and nonlabor income (equal in part to a rate of return on net worth) be W and Y and denote $P=g(W,Y)$—ignoring the age variables. The labor-supply function may be written: $L=f(W,Y,P)+u$—ignoring all other variables. The explicit effects which are measured are:

$$\frac{\partial L}{\partial W}=f'_W;\ \frac{\partial L}{\partial Y}=f'_Y;\ \frac{\partial L}{\partial P}=f'_P,$$

which are all constants in a linear and additive regression model. However, since $P=g(W,Y)+e$—ignoring age—and since the functional form of the labor-supply equation is such that we can write: $L=h(W,Y)+g(W,Y)+u$, the total effects of the wage and income variables are:

$$\frac{\partial L}{\partial W}=h'_W+g'_W;\ \frac{\partial L}{\partial Y}=h'_Y+g'_Y.$$

The Greenberg-Kosters claim is that

$$\frac{\partial L}{\partial W}=h'_W=f'_W \text{ and } \frac{\partial L}{\partial Y}=h'_Y=f'_Y$$

insofar as wage and income effects are being measured net of preferences; which is to say that the g' components of the total effects are assumed to represent preferences.

[18] As an alternative explanation of the measured positive coefficient on the preference variables, consider that predicted net worth may be representing "permanent income." As seen in the equation for P, above, "preferences" is negatively related to predicted net worth (or "permanent income"), and an underlying negative relation between permanent income and labor supply would rationalize the measured positive effect between preferences and labor supply.

Another point of reconciliation may lie in the different samples used. The Fleisher et al. study is restricted to older workers. Greenberg and Kosters cover the range, and when they run regressions for males aged 55 and over, indeed, the "preference" variable, though positive, is insignificantly different from zero and therefore not very different from the relation estimated by Fleisher et al. One could argue, even within the framework suggested by Greenberg and Kosters, that a preference for asset accumulation may be negatively related to labor supply for older workers. If asset accumulation, per se, represents a preference for future goods or future leisure relative to current goods or current leisure, then among individuals with the same income-earnings abilities and the same life expectancies we should expect a positive partial regression coefficient between asset accumulation and work at young ages and a negative partial coefficient at older ages. Intuitively, someone with a desire to retire early (or "slow down" by means of longer vacations, and so forth) ought to accumulate assets in his early years (that is, work more) and enjoy leisure (work less) in his later years.[19]

Skills in homework productivity constitute another unobservable variable which may, for wives especially, lead to a biased estimate of the wage effect. The problem is illustrated by reference to Hall's specification of a high positive correlation between the market wage of the wife and her home productivity (or home wage). If, as seems plausible under conditions where other things are equal, the home wage is positively related to time spent at home, and thereby negatively related to time spent in market work, then the observed market-wage/market-work relation is a biased (downward) measure of the relation between a wage change produced by an income maintenance program and the subsequent change in market work. The legislation will change the market wage but not the home wage, so only the partial effect of the former, net of its covariation with the latter, is what we are after. An independent measure of the return from this most prominent alternative productive capacity is badly needed.

Similar comments could be made about the covariation that exists between market wages and fringe benefits and/or nonpecuniary conditions of the job on the one hand, and between fringes and nonpecuniary conditions and the quantity of labor supplied on the other hand.[20] Since income maintenance programs change only market wages, it is the partial effect of this variable, holding fringes, et cetera, constant, that is needed to estimate the effect of the type of legislation on labor supply.

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[19] By contrast, the bequest motive for savings produces a positive relation between work and preference for asset accumulation over all ages—certainly fitting the Greenberg-Kosters formulation and rationalizing a lifetime allocation of more time to work and less to leisure.

[20] The empirical correlation between market wages and fringe benefits appears to be positive, which suggests that the measured wage/labor-supply relation is upwardly biased on this account. The amount of fringe benefits is a substantial fraction of the wage bill nowadays, and it has increased steadily in the recent past. The Bureau of Labor Statistics reports that about 18 percent of "total compensation" per employee in the private nonfarm economy in 1968 was for fringe benefits (mainly leave time, and retirement, health, and unemployment benefits). (See Alvin Bauman, "Measuring Employee Compensation in U.S. Industry," *Monthly Labor Review*, October 1970, pp. 17–24.) A positive correlation between the amount of fringe benefits and hourly wage rates has been found rather consistently. See Robert G. Rice, "Skill, Earnings, and the Growth of Wage Supplements," *American Economic Review*, vol. 56, May 1966, pp. 583–593; also Albert Rees and George P. Shultz, *Workers and Wages in an Urban Labor*

It may be noted here that Hall's assumption of a positive correlation between the home wage and the market wage among wives was connected with his decision to compute that portion of the family's whole income attributable to the wife by multiplying the wife's predicted wage by 2,000 hours. Other authors use the mean of reported hours worked (about equal to 800) to fix the points of income compensation for wives. Since the substitution effect

$$\left(\frac{\partial h^*}{\partial W}=\frac{\partial h}{\partial W}-\frac{\partial h}{\partial Y}\bar{h}\right)$$

is larger the larger is $\bar{h}$—the equilibrium value of hours worked, the fact that Hall uses 2,000 hours for this value naturally contributes to the relatively large substitution elasticity which is computed for wives from his regressions.[21]

Returning to the fundamental problem of heterogeneity in preference and/or capacities for market work among the persons surveyed, let us note that the principal device for obtaining more homogeneity is to omit various categories of persons—members of high-income families, members of families on welfare, persons not in the labor force, et cetera—from the regressions. We have already mentioned the difficulties in estimation when the observations are truncated on the basis of income. In addition, the more homogeneous the group the less, generally speaking, is the variability in the wage and income variables. This is undesirable in general because of the loss in efficiency of estimation; but what makes it particularly damaging in investigating the labor-supply effects of income maintenance is that relatively large extrapolations outside the sample concentration of values for wage changes and changes in nonlabor income are required if the estimates are to be applied to the large changes resulting from the programs.

The need to preserve a good deal of variability in wages (especially) was one motivation for Hall's and Boskin's decision to eliminate from their labor supply equations a number of variables, like education, health, age, and others, which are correlated with wages. (Indeed, these variables were linear determinants of the predicted wage variable used in the labor-supply equations.) The main problem with this procedure is that it rests on the assumption that the variables—education, health, age, et cetera—are not related to labor supply in their own right, or independently of their effects on labor supply via their relation to wages. But some a priori arguments for expecting independent

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Market, University of Chicago Press, Chicago, 1970, pp. 77–79, and their citation to other studies.

The correlation between nonpecuniary aspects of employment and wages is more difficult to ascertain. The conventional theory of wage differentials, which views wages as a source of compensating differentials, suggests a negative correlation, whereas the prevalence of noncompeting groups, in the Mill-Cairnes sense, rationalizes a positive relation. It is, of course, the empirical relation in the sample under investigation that determines the direction of bias in the measured wage effect.

[21] See table 2.

effects are in fact easy to advance, and the empirical evidence from studies which do include these variables in the labor supply regressions also argues for their inclusion. The main point we wish to make, however, is not that it is correct or incorrect to exclude these variables from the labor supply equations. The main point, rather, is that the limited variability in wages (or predicted wages) found in nonexperimentally-generated data tends to "force" some authors to impose such a priori restrictions on their models.

This clearly illustrates a basic limitation inherent in cross-sectional data—namely, that for observationally equivalent workers, the variety of demand conditions may not provide a sufficiently wide array of wage levels to permit estimation of workers' response to a drastic exogenous cut in net wages, that is, the 50–70 percent implicit tax rates that proposed income transfer programs would apply to earnings of the newly subsidized working poor. Similar problems exist for the income effect: Is there enough variation, holding constant all the things that need to be controlled in unearned (and not means-tested) incomes, to shed light on what would happen if an income guarantee were introduced which was large enough to permit existence without work? In both cases a great deal of reliance must be placed on extrapolations well beyond the bulk of observed situations. Hence, the form of that estimated relation is crucial.

C. Errors in Variables

The device of obtaining a predicted wage in a first-stage regression and then entering this variable in place of the reported wage in the second-stage labor supply model, as was done by Kalachek and Raines, Hall, and Boskin, illustrates an attempt to deal with the problem of measurement error in the wage variable. We have previously noted that the definitional relation between hours spent at work and the measured wage rate produces a simultaneity problem when the wage rate is measured with error. The authors who use the two-stage device argue that their predicted wage is more accurate than the wage reported in surveys as a determinant of the labor supply measure for a whole year.

We would like to make two points here. First, it is not known whether reported wages measure the theoretically desired "normal" or "permanent" wage less accurately than does the predicted wage. Some portion of the variation in reported wages is undoubtedly sheer error or noise; but another portion is attributable to experience, training, quality of education, and many other real components of wage variation, all of which are not included as variables in the regression models that predict wages. Indeed, the variables that are included account for only a small fraction (around 15 percent) of the variation in reported wages. Second, it would be interesting and useful to measure the effects of wage variation on labor supply for persons of a given age, education, health status, and so on. What the device of predicted wages does, however, is to suppress all such variation for those groups and, essentially, assign the within-group mean to all observations in the group. Thus, the entire wage effect is, in fact, an effect of the various demographic characteristics defining the cell. All of which points again to the senstitivity of the assumption that some or all of the variables used to determine the wage have no independent effects on labor supply.

Errors in income reporting are a well-known and much lamented problem in empirical research in economics and need little discussion here. Suffice it to note that, as with the wage rate, there are really two measurement problems: selecting the right concept of income, and measuring that conceptual variable accurately. The history of the permanent income hypothesis illustrates the first issue, and the persistent efforts by survey and census takers to cope with the pervasive under-reporting of income, especially that from nonlabor sources, attests to the second. To the extent that errors in measurement are random, the effect is to bias the estimated coefficients toward zero. Investigators used varying strategies to deal with this source of bias.

D. Some Speculations About the Interpretation of the Differences Among Studies

The foregoing discussion points up the large number of discretionary choices investigators must make when specifying models to estimate income and wage parameters—choices about the sample selected, the time period covered, the definition of the labor supply variable as dependent variable, which independent variables should represent the negative income tax plan's transfer payments and induced wage rate changes, and which among the many possible control variables should be used. Clearly, the larger the income effects and substitution effects produced by an estimation procedure, the larger the predicted work reductions will be for any given income maintenance plan, which would increase recipients' nonwage income but lower their effective wage rates. The following specifications operate to increase the negative income effect (less work and more income) and the positive substitution effect (more work with higher wage rates, but less with lower wage rates which make leisure relatively cheaper):

(*a*) Retention of persons in the sample with some work-conditioned (but unearned) income;

(*b*) Retention of persons who are obviously out of the labor force;

(*c*) Truncation of the sample to exclude observations with current incomes above a certain level;

(*d*) Exclusion from the model of variables such as education, age, health, and others which might be presumed to hold "tastes" constant—an objective which, as mentioned earlier, is one justification for excluding persons (especially male heads-of-household) with zero hours of employment; and

(*e*) In addition, there is the obvious point that increasing the accuracy and completeness of the wage-rate and non-work-conditioned income variables serves also to increase the measured effects of these variables.

Looking back to tables 1 and 2, one can observe that the sharpest contrast in parameter estimates and the labor supply effects they imply is between the study of Kalachek and Raines, on the one hand, and those of Garfinkel and Cohen et al. on the other. The former study differs from both the latter two in terms of the model specification regarding at least the first three of the points made above. To decide which set of assumptions is correct, of course, one must know which experiment implied by the sample most closely represents the experiment of a negative income tax in future years.

VI. LOOKING AHEAD

Although the above review of problems is sobering, it should not be interpreted as disheartening. Solutions are within reach for many of these problems, and some of the others do seem intrinsically no harder than problems already solved.

It is clear, however, that work which deserves to be called authoritative in this area requires a very heavy input of time, ingenuity, and resources. The studies in this volume testify that we are beyond the point where a clever insight can be combined with available scraps of data in an afternoon session with the computer to produce results that add to our understanding of labor supply issues. Even with the substantial talents and industry these authors evidence, their studies fail to provide clear and consistent guidance for policy decisions. Major improvements on these studies will require more refined data, more complicated models, a heavier input of computer technology, and probably larger and more concerted efforts on the part of research groups.

As usual, it is impossible to say whether more (or more appropriate) data are a greater need than more (or more appropriate) theory. The theory needs the inspiration and clues that come from groping empirical efforts, and the very definition of an ideal data set requires a highly structured analytic framework.

It is clear that, insofar as we limit consideration to the most basic micro observations at specific points or over short intervals of time, the ordinary linear regression model has serious shortcomings. Institutional constraints do limit the choices available to workers and render the smooth continuous opportunity locus so dear to economic theory quite invalid as a literal representation of reality. The traditional solution to this problem has been to look at labor force participation as a discrete binary variable—to work or not—which then conditions the more continuous measures of labor supply—how much to work. But in this, as in many other areas, econometricians have fallen into the quick, easy, and unfortunately heavily precedented practice of using the same ordinary least squares linear regression model.

Better statistical models exist for the simple dichotomous variable (for example, probit, logit, et cetera),[22] and there are also models which can accommodate mass points at prespecified levels (such as 0 and 40 hours per week) along with scattered intermediate values. Certainly these procedures involve more complicated and expensive estimation techniques, but the absolute computation cost has gone down so dramatically in recent years that cost is no longer so convincing an explanation as is inertia on the part of analysts. When observations can be made over longer periods of time, such as a year, measures of cumulative or average performance come closer to fitting the simple textbook example. But even within that framework corner solutions are quite respectable, and for many identifiable classes of potential workers (for example, wives, teenagers, retirees, et cetera) this outcome is the predominant one. Once again the ordinary linear regression model suffers.

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[22] For an introduction to these models and citations to part of the extensive literature on the subject, see Arthur S. Goldberger, *Econometric Theory*, John Wiley & Sons, Inc., New York, 1964, pp. 248–255, and H. Theil, *Principles of Econometrics*, John Wiley & Sons, Inc., New York, 1971, pp. 628–36.

If one proceeds toward models that treat joint household decisions in what appears an appropriately simultaneous way, and additionally imposes restrictions that embody the minimal tenets of economic theory, the statistical techniques appropriate to the stochastic specification of these models will require simultaneous equations techniques that are capable of incorporating in a flexible way a variety of possibly intricate restrictions on coefficients and residual moments. For the most part the statistical theory as well as computer algorithms exist for these more complex estimation problems. What is needed is more creative theory building and application.

Important beginnings have also been made on theoretical models that analyze allocation of time within a more comprehensive framework, recognizing nonmarket as well as market activities, as well as the complementarity of market goods and time for leisure and other activities carried on within the household. The work component in various forms of investing in human capital formation, such as education and migration, has received considerable attention by economists, but the work component in household activities has gone almost unstudied in any systematic way. This latter analysis requires kinds of data that have not yet (and perhaps can never be) collected on a massive scale.

Turning to the question of the quality of data, there is a strong case for more analysis of existing panel data in the immediate future. Such data appear to hold a good deal of promise, but they have been insufficiently analyzed to date and the problem of nonresponse or panel attrition will require careful attention—even more than what is needed but seldom forthcoming in dealing with nonresponse in cross-section surveys. From this process we can expect improved theoretical and econometric models along with a more precise specification of data shortcomings that could be remedied in subsequent survey work.

The studies just reviewed also indicate several ways in which data collected can be improved in the immediate future. We have advanced considerably in understanding how important it is to identify the sources of nonlabor income and the conditions involved in the receipt of such income, but further refinements are needed. How much of one's wealth is inherited, and how much is the result of savings from previous work? What constraints on work come with pension income or public assistance?

Better information on wage *rates* stands out as another important need. A record of previous wage rates (during the past year or so), a prospective pattern of expected wage rates, and some measure of the monetary equivalent of fringe benefits would permit a much sharper measure of the opportunity cost of time, and these data inputs do appear within the present capacities of survey techniques.

There is a need for methodological research on improved methods to determine the quantity of labor supplied over periods at least as long as a year. If we are to obtain measures of labor supply independently of the wage rate, direct questioning about hours (such as is done in the panel survey used by Fleisher et al.) appears necessary.

There is also the question of how best to conceptualize and measure labor force activities in a way that includes job search when not employed and, perhaps, excludes leisure activities when employed. Consider, for example, the work choices of small entrepreneurs, farmers, and independent single "contractors" such as some building tradesmen, domestic employees, barbers, and the like. For the most part these groups pose problems that are too difficult within the traditional work/leisure framework and have hence been either set aside or glossed over in large and amorphous aggregates.

Of course, further insights and better models, as they are developed, will also enable researchers to approach the ordinary large scale cross-sectional labor force survey with more prior constraints. Combined with more accurately measured variables, the much greater statistical power inherent in large sample sizes will be achieved.

One limitation, however, cannot be overcome: the problem that some events which are uniquely suitable for making inferences about some aspect of behavior just do not occur spontaneously, or occur so infrequently that it is not feasible to find them by a sampling procedure. In such cases it is clear that passive observation of nonexperimental events will not do, and the only alternative is to induce such events deliberately.

Experimental research does open up a lot of new possibilities for obtaining information efficiently. Prior knowledge can be used to structure an experiment to focus on specific information needs. The range of variation of key variables can be controlled either by direct treatments or by stratification of the sample. It is possible to get causal inferences in much more direct ways, and so on. But this is not the place to engage in a full discussion of the merits and weaknesses of experimental research. Suffice it to say that experimental research has just begun and some difficult problems and limitations are apparent.

To note a few: Experiments are costly and hence are likely to be short, relative to the more permanent changes they try to simulate.[23] The experimental studies now underway are even less equipped to address the complicated questions of general equilibrium than are nonexperimental research studies. They operate only on small and localized samples of the relevant labor force, and the "treatment" effects *may* be critically dependent on the fact that not *everyone* in their income stratum is given the same treatment. Some of the more complex market processes could be observed experimentally if one could experiment with *entire* labor markets. But this requires both more money than has been available and, perhaps, more careful specification of precisely what should be observed and how the required measurements could be made. Some substantial part of the full general equilibrium will remain out of reach, simply because a full-scale implementation would be needed to generate data even if the relations at that level could be modeled with enough stability to be useful. Experimental research is slow, and several years may elapse between the beginning of an experiment and its final report. Much more knowledge of dynamic adjustment processes is needed to use the data gained, especially when the experiment is short. Finally, there is the perpetual problem of spurious responses to the special treatment constituted simply by being included in an experiment—the notorious "Hawthorne" effect.

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[23] The question of biases in estimates of effects of "permanent" changes on the basis of short duration experiments has been discussed by Charles E. Metcalf, *Making Inferences From Controlled Income-Maintenance Experiments*. Institute for Research on Poverty Discussion Paper No. 103–71, September 1971.

For all of these reasons and more, experimental research should properly be regarded as a last resort—a possibly feasible solution to problems that cannot otherwise be resolved. And, for the foreseeable future, progress in understanding such basic economic relationships as labor supply will depend heavily on nonexperimental research.

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